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Max Scharnberg

**The Non-Authentic Nature  
of Freud's Observations**

Vol. I: The Seduction Theory

UPPSALA 1993

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### Abstract

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The most original feature of the present approach is its pervasive lack of originality: Freud's texts are treated *EXACTLY* like science is used to treating non-psychoanalytic texts. Textual analysis, an objective tool, reveals that all Freud's non-trivial observations are faked. As for the 18 patients of 1896, whom Freud erroneously considered hysterics, Freud was the exclusive originator of the idea that they had been seduced. No patient recalled any hitherto repressed event, and no patient was cured. Some patients, probably 14, never existed at all.

The major part of the third seduction paper consists of persuasive techniques to distract the reader's attention away from the almost total absence of clinical observations (4,9% of the paper). The seduction interpretations are constructed by means of *the principle of similarity*. Like traditional superstition explains a hare-lip by assuming that the pregnant mother had been scared by a hare, so Freud explains oral eczema and a stiff leg as the result of fellatio having been performed in the cradle, or the foot having been used as a masturbation tool by an adult female during preschool age.

Freud eventually retracted the seduction theory because he was incapable of fabricating any concrete observations, since he did not know what kinds of data *could* prove his constructions.

Many aspects of the case-study of Dora are analyzed. They too reveal Freud's lack of knowledge of human nature, his low capacity for clinical observation, his deliberately faked or distorted observations, and his dogmatic prejudices. All psychoanalytic observations are trivial, and are immediately available to any untrained lay man outside the psychoanalytic situation. Various features of the published texts unambiguously reveal that the secret observations had exactly the same character as the published ones.

Later development such as ego analysis, object relationism etc. do not remedy any of the original defects, but add more devastating ones. During a century any innovation has been associated with the decrease of the empirical content of the writings.

Since 1960 the existence and high efficacy of behaviour therapy could no longer be concealed. A world-wide hateful campaign against the latter failed. Psychodynamic therapists saw their practice diminished. Looking for new markets, they dug out Freud's fraud of 1896 to "prove" their competence in handling cases of sexual abuse of children. The Freudian variety of deductions (oral eczema proves fellatio) is used to send flagrantly innocent people to prison. Psychiatrists commit perjury, and judges and jurors uncritically accept their pseudo-evidence. Seventeen recent Swedish trials are analyzed.

**Key words:** Freud, psychoanalysis, seduction theory, Dora case, pseudo-science, textual analysis, sexual abuse of children, lying

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## Preface to Vol. I and II

*It is a sensational historical event that, for the first time in Sweden during generations, proponents of psychoanalysis did not succeed in stopping a research report devoted to the objective investigation of their favourite theory.* There has been no want of attempts in this direction, and they very nearly attained their aim. Moreover, they did succeed in postponing the publication of this report by more than half a decade.

I want to make it absolutely clear that the following comments are not intended as criticism against any particular institution. The problem is world-wide. No country can be named in which more than a few scientific studies have been published between 1945 and 1980—and in many countries such studies are completely absent. Were no reports produced? Did many behavioural scientists realize that their career would be endangered if they applied normal scientific standard when evaluating psychoanalytic writings? *Or:* were a non-neglectible number of manuscripts of a high quality stopped by a few individuals in key positions?

All over the world, foundations supporting research apply a policy which makes things easy for certain kinds of people. They do not think that true believers in Genesis, spiritualism and nazism are most competent of evaluating the scientific merits of reports on geology, spiritualism and nazi crimes. But they do think that psychoanalysts are most competent of evaluating reports on psychoanalysis. Implicitly, psychoanalysts are given the privilege of deciding what kinds of criticism of their theory should be permitted.

I may supply a concrete illustration of the kinds of arguments used to justify the rejection of a manuscript. *The report should not be printed because the writer is ignorant of universally accepted scientific methodology, according to which it is irremediably impossible to disclose whether two published observation statements contradict each other, or whether a certain conclusion is justified on the basis of a certain set of published data claimed to prove it—UNLESS one has access to the private files of the writer.*

No less than three of the reviewers have unanimously advanced exactly this argument. Strangely, they think they are capable of evaluating my text without having any access to my files. In other contexts all three have likewise testified on the *non-contradictory* nature of Freud's published observation statements and on the *validity* of his conclusions, despite their lack of access to Freud's files.

It is not altogether easy to understand how numerous academicians, whose genuine competence and honesty cannot be doubted, are nevertheless prepared to accept such strategic pseudo-arguments. It is also a little surprising that they did

not realize that the specimen is a mechanical application of one of B. Farrell's numerous attempts at making psychoanalysis immune to criticisms which he feels are impossible to answer.

It may be worthwhile to study the handling of manuscripts on psychoanalysis, which have been rejected by foundations all over the world. Although a reviewer may have used a more coarse strategy (e.g., to substitute every statement and argument actually found in the manuscript with fictive and imbecile ones), the reviewer's pronunciation alone might often reveal that he has invented an idiosyncratic and absurd methodology and has—*correctly*—shown that the writer has not applied the latter. I wish to emphasize that numerous foundations have reason to reconsider their routines for choosing reviewers to evaluate manuscripts on psychoanalysis.

The two volumes published now are intended to form the beginning of my *chef d'œuvre*, which may finally comprise more than 20 volumes (first drafts exist of more than a half a dozen ones). The *Swedish Council for Research* has accorded a generous grant for printing costs and linguistic correction.

Likewise, I have received the most generous assistance from libraries and other institutions. (Only the Swedish Association of Psychoanalysis refused to answer my letters, which were concerned with the most trivial and neutral questions.) I want to express my deep gratitude to all the following enterprises:

*in Stockholm:* The Royal Library; The Psychological and Educational National Library; The Library of the University; The Library of the Carolinian Institute; The Swedish National Archive of Recorded Sound and Moving Images; The Archives of the Labour Movement; The Library of the Royal Dramatic Theatre; The Swedish Parliamentary Ombudsman; The Municipal Library (where the branch in Kista should not be forgotten); The Goethe Institute; The Cultural Center of the German Democratic Republic (no longer in existence); The American Cultural Center (no longer in existence); The Fulbright Commission; The Nobel Library of the Swedish Academy; The Nordic Museum; The Swedish Broadcasting Commission; The Library of the Telephone Company;

*elsewhere in Sweden:* the libraries of the universities of Lund and Göteborg; Carolina Rediviva, Uppsala;

*in Denmark and Finland:* The Royal Library, Copenhagen; the libraries of the universities of Aarhus and Odense; the municipal library of Elsinore; Kuopio University Library, Kuopio;

*in Germany, Austria and Switzerland:* Staats- und Universitätsbibliothek, Hamburg; Staatsbibliothek, Preussischer Kulturbesitz, Berlin; Freies Universität, Berlin; Technische Bibliothek, Berlin; Erzbischöfliche Akademische Bibliothek, Paderborn; Universitäts-Bibliothek, Freiburg im Breisgau; Universitätsbibliothek, Heidelberg; Bayerische Staatsbibliothek, München; Universitätsbibliothek, Wien; Schweizerische Landesbibliothek, Bern;

*in Anglosaxon countries:* British Museum, London; British Library, Boston Spa; Library of Congress, Washington; University of Alabama Libraries; University of North Carolina Library; Carleton College Library, Northfield;

*other countries:* Statni Knihovna, Praha; Bibliothèque Nationale, Paris.

Among all librarians I shall specifically mention John Rohnström at the Royal Library, one of the utmost learned men in Sweden.

The Editorial Office at Uppsala University has spent labour in excess of normal obligation upon my research. Donald Luscombe has corrected my English.

The dedication of the first volume to Joseph Wolpe and Stanley Rachman, and the second to Karl-Georg Ahlström, is a most natural choice. Wolpe and Rachman were the first to apply textual analysis to Freud's writings. Their re-analysis of the case-study of Little Hans is the most important paper ever written on psychoanalysis. They have invented the fundamental methodological approach applied throughout *The Non-Authentic Nature of Freud's Observations*.—Ahlström has, because of his pervasive generosity rather than because he shared my views, supported my research during many years.

Without the intervention of the professors Lennart Sjöberg and Germund Hesslow, the psychoanalytic resistance would probably never have been conquered. But Hesslow has made one more contribution. I shall have to admit that the repeated rejection of the manuscript, which was repeatedly justified by the most surrealistic arguments, and which necessitated repeated re-working of aspects which were in no need of any re-working, finally lead to a text suffering from elephantiasis, in which more space was devoted to the refutation of inappropriate objections than to the central topics of the report. Professor Hesslow has devoted an immense labour to the task of making the manuscript readable to an unbiased reader.

As for the Third Book, the attorney Peter Haglund, Falköping, has contributed with invaluable advice. In trial after trial he has fought for flagrantly innocent fathers and stepfathers falsely accused of incestuous assaults, as if the fate of his own brother were at stake—often against psychiatrists using Freud's fraud to construct fake-evidence. I do not think Haglund has his equal in Sweden.

M.S.

**First Book**  
**Methodological**  
**Preliminaries**

# The Policy of the Open Door

*Blessed are those who have not seen and yet believe.*

John 20:29

§1. The present report, of which two volumes are published now and six further volumes are nearly completed, may well turn out to be my *chef d'œuvre*. I shall postpone the description of the main problems and tasks until chapter 12. And despite the fact that the basic methodological approach, i.e. *the technique of textual analysis*, will not be explained until later volumes, I shall start with a brief description of the most original feature of my report. I do acknowledge the existence of a microscopic number of admirable exceptions to the following generalization. But apart from these, *The Non-Authentic Nature of Freud's Observations* is altogether dissimilar from any other paper on psychoanalysis ever produced.

Ironically, the unique nature of the report derives from its *lack of originality* as to the fundamental methodological procedure applied. Psychoanalytic texts are here treated in *exactly* the same way in which behavioural scientists have always found it appropriate to treat *non-psychoanalytic* texts.

§2. I do not merely recommend a certain approach. I claim that the latter is mandatory to any adequate study of any psychoanalytic text; and that it is so not only for researchers but also for readers.

The concept underlying the approach must have a name that adequately indicates its character; that is conspicuous; and that is easy to remember. I have chosen the name *the policy of the open door*.

The opposite concept may be termed *the policy of the closed door*. Far more than 99% of all psychoanalytic writings and papers on psychoanalysis are based on the latter.

Both concepts will be more extensively discussed in a later volume. Nonetheless, the reader must from the very start be familiar with the basic distinction.

§3. I shall quote one international and one local excerpt, both of which are *typical* of psychoanalytic and psychodynamic texts.

"Freud, who was responsible for drawing attention to the importance of childhood sexuality, arrived at his conclusions through the analysis of adults. The analyst has a unique experience every time he conducts a successful analysis, in that he sees unfolding before him the patient's childhood and infancy as it appeared to the patient. He has the repeated experience of getting to see the natural history of a psychological disorder, with all the interweaving of the psychological and the physical, of the personal and the environmental, of the factual and the imagined, of what has been conscious to the patient and what has been under repression.

In analysis of adults Freud found that the foundations of their sex life and sex diffi-



culties went back to adolescence, and back to childhood, especially to the two- to five-year-old period.

He found that there was a triangular situation which could not be described except by saying that the little boy was in love with his mother, and was in conflict with his father as a sexual rival. The sexual element was proved by the fact that it was not just in fantasy that these things were happening; there were physical accompaniments, erections, excitement phases with climax, murderous impulses, and a specific fear—fear of castration. This central theme was picked out and called the Oedipus complex, and it remains today a central fact, infinitely elaborated and modified, but inescapable. Psychology built on a hushing up of this theme would have been doomed to failure, and therefore one cannot help being grateful to Freud for going ahead and stating what he repeatedly found, bearing the brunt of public reaction.” (Winnicott, 1975:148) [Q-3:1]

[Gestalt therapy] “has shown itself to be most effective with inhibited individuals who are distinguished by extreme self-control—individuals who may be characterized as neurotic; who suffer from phobias; who are perfectionists; who have difficulties in making social contacts; who are depressed; etc. [...]

Gestalt therapy is built, to an extent unequalled by any other type of therapy, on this [...] sensitivity to the body language of other people. [...] [Gestalt therapeutic technique] is easy for non-trained persons to imitate [...] But when the therapist is wanting in psychological background knowledge and personal sensitivity, he will be unable to guide the therapeutic session in accordance with any understanding of what is important or not to the patient.” (Lundh & Leymann, 1981:78. The former writer is solely responsible for the quoted text. Transl.) [Q-3:2]

§4. *Both quotations have essentially the same logical structure.*

*Each of them consists of nothing but dogmatic assertions about the nature of the empirical world,*

*together with the categorical postulation that the truth of all these assertions has been conclusively verified.*

*None of the quotations contains the slightest trace of supporting evidence, however weak or unsatisfactory.*

*Both writers have PROVED THEIR CLAIMS BY THE INVOCATION OF SECRET EVIDENCE.*

*Whether the reader be skeptical of the claims, or he be prepared to believe all assertions, he is deprived of any possibility of investigating for himself the support of the claims, and of making an independent evaluation of the veracity of the observational claims and the appropriateness of the derivations.*

*Each writer simply expects the reader to trust the former’s judgement, while the latter is not given the least information about the nature of the empirical backing and the procedures applied for arriving at and justifying the conclusions on the basis of this backing.*

The features just listed may be said to define the police of the closed door. The reader is, as it were, placed outside a closed door, and is screened off from the possibility of learning anything about what goes on on the other side of the door and, consequently, whether or not any of the claims are indeed true. (In later volumes I shall describe a number of complex techniques by means of which the

truth may sometimes be extracted from deliberately faked reports. But this circumstance is of course not relevant.)

Texts like Q-3:1 and Q-3:2 should not be permitted in scientific communication. It is a regrettable state of things that thousands of them are acknowledged by the academic community as providing genuine scientific evidence.

§5. Q-3:1 and Q-3:2 together contain a total of one single sentence that is not grossly false, viz. the following: [Gestalt therapeutic technique] "is easy for non-trained persons to imitate" (from Q-3:2). As for some of the assertions, it has been thoroughly documented by Ellenberger (1970), Sulloway (1979) and others that Freud did not at all *invent* the ideas of childhood sexuality and of the close causal connection between sexuality and neurosis. He *stole* these ideas from others, and then accused the others of having stolen them from him.

Freud and his followers have never *discovered* or *observed* anything pertaining to childhood. Nor has any psychoanalyst ever seen "unfolding before him the patient's childhood and infancy", despite their firm testimonies. In fact, Winnicott dares not stick to his own claim, but adds a hidden reservation which will make the claim immune to falsification: it is not a matter of the childhood situation as it actually occurred, but "as it appeared to the patient".

§6. I shall supply a few illustrations of the true reality behind Winnicott's postulations—a reality which the reader has no chance of suspecting from his text. Most of the examples will be further discussed elsewhere.

Freud's theory is, just like the most recent varieties of psychoanalysis, through and through based on a causal principle borrowed from primitive superstition: *the cause is similar to the effect*; hence, the cause of a phenomenon may be deduced by the postulation of an event that is similar to the effect. Cf. the idea that the hare-lip is caused by the pregnant mother having been scared by a hare.

In his letter of 3.1.1897 to Fliess, Freud described the case of G.de B. "She is suffering from eczema around her mouth and from lesions that do not heal in the corners of her mouth." It never occurred to Freud that some microbe or allergic reaction might be responsible for the symptom. From the symptom alone he deduced that G.de B. had, during infancy, been sexually seduced: her father had applied his penis in her mouth.

"When I thrust the explanation at her, she was at first won over; then she committed the folly of questioning the old man himself, who at the very first intimation exclaimed indignantly, 'Are you implying that I was the one?' and swore a holy oath to his innocence.

She is now in the throes of the most vehement resistance, claims to believe him, but attests to her identification with him by having become dishonest and swearing false oaths. I have threatened to send her away and in the process convinced myself that she has already gained a good deal of certainty which she is reluctant to acknowledge." (Freud, 1985:220f.) [Q-6:1]

§7. *This example is paradigmatic of Freud's entire interpretative endeavour during his whole life.*

The reader should pay attention to many aspects of the text. First, the infer-

ence is pseudo-scientific, and could only by means of a miraculous coincidence be true. Second, Freud's diagnostic competence is incredibly very low. Third, his claim of having completely cured 18 patients out of 18 attempts, of *such symptoms* (inter alia) by means of *such* interpretations, is simply unbelievable. Not even the most gifted suggestive therapist could accomplish such a result.

Fourth, Freud has not merely made a (great) mistake. He has reverted the development of psychology to the stage of magical prejudice. Fifth, he reveals, here like everywhere else, his lack of knowledge of human nature. He is *surprised* at the fact that the patient told her father what she had learned; he even applies the word "folly" about her behaviour.

Many readers may miss the fifth point. It is a recurrent feature of psychoanalysts' interpretative activity, that they are incapable of distinguishing between the aspects implied by their theory, and aspects derived from their own momentarily prejudices and carelessness. (We shall later encounter certain magnificent illustrations, not least one supplied by Bonaparte, 1945.) If psychoanalytic theory (in the version of 1897) were true, Freud would be entitled to infer that *some male or other* had practiced fellatio upon the patient. But psychoanalytic theory will provide no cue as to the identification of the person. Freud might *legitimately* have evaded the conflict with the patient by means of a rather unimportant modification of his interpretation: the "seducer" might have been any male, related or not.

§8. Sixth, the example throws much light upon Freud's emphatic and repeated assurance that the infantile seduction events were entirely and completely invented by the patients, while Freud was reluctant to acknowledge such recount to be genuine recollections. Seventh, much light is also thrown upon Freud's later excuse that he had, inter alia in 1897 and in relation to G.de B., merely been gullible and had believed in *his patients' "fabulations"*.

Q-6:1 is as illuminating as regards Freud's vehement assurances during his entire life, that suggestive influence is altogether absent from psychoanalytic treatment. Flagrantly, Freud applies the most coarse and primitive persuasive measures to force the interpretation upon the patient.

The case of G.de B. is no exception. It is easily seen that: in the case-study of Dora, Freud behaves like a hyper-authoritarian and hyper-brutal police officer who presents his conclusions before any evidence has emerged, and then puts Dora under very hard pressure in order to extract a *confession*; as we shall see, Freud himself uses the word "confession", a word which in German cannot be used in the sense of a spiritual confession but only in the police sense.

§9. Dora was 17-18 when Freud treated her. Most commentators agree that Freud saw "unfolding before him the patient's childhood and infancy". Inter alia, he *observed* that she had masturbated when she was 8 years old, and had abruptly stopped at that age as the result of one single anxiety provoking event, viz. the spying event (which will be described in Q-17:1). What is the truth?

First, the age of 8 is not in the least derived from any clinical observations, but from *magical numerology*. A comprehensive account of Freud's deductive procedure is found in his letter of 6.12.1896 to Wilhelm Fliess.

Freud pays no attention to individual differences, but takes the period of pregnancy to be exactly 276 days. Without any justification he combines this period with the magical number 12. And *12 times 276 days* after conception will be equal to about 8 years 4 months after birth. This is the sole reason why the age of 8 is considered a critical age.

Second, this critical age is referred to over and over again throughout Freud's writings. I shall select only a few examples. In the third seduction paper of 1896—which constitutes the primary text to be analyzed in the first volume of the present report—we are told that sexual seduction after the critical age could not possibly, as far as Freud has found out, lead to any psychopathological reaction. (This is one more illustration of Freud's lack of knowledge of human nature.) Likewise, in the case-study of Dora we are told that children usually cease to masturbate when they are 8, as the result of one single anxiety-provoking event. In 1900–1905 Freud allegedly made an infinity of clinical observations to the effect that children usually masturbate *during the latency*. When he later looked back on all his previous clinical observations, including the ones obtained during 1900–1905, he found the empirical generalization that children never masturbate at the age of 8.

§10. Third, if Freud's description is true, Dora experienced next to nothing when she was 0–7 and 9–11, while a wealth of events were concentrated to the age of 8. This circumstance alone would suggest that Freud had mechanically added the age indication whenever he was told of any event occurring at some indeterminate time during childhood.

A standard device for saving Freud's competence would be the ad hoc hypothesis that Dora was responsible for the clustering. But if she habitually said, "This happened when I was 8", then Freud did not notice that her presentations were very strange. It is improbable in the extreme that two persons destined to make exactly the same highly unusual mistake, would meet and, independently of each other, would make them in the same situation.

One further circumstance is decisive. It is logically and empirically impossible that Dora experienced everything attributed to her by Freud. For instance, she cannot have partaken in mountain climbing at a time when she had injured her foot so severely that she could hardly walk.

§11. Fourth, one of Freud's proof of Dora's childhood masturbation is thoroughly based on the *principle of similarity* explained in §6. She wet the bed when she was 7–8. And bed-wetting is *similar* to nocturnal seminal emissions in adult males. This circumstance proves that bed-wetting is a masturbation equivalent—also in young girls (GW-V:90/SE-VII:190). Freud maintains that bed-wetting is *caused* by masturbation. And he is allegedly in the possession of a wealth of clinical observations showing that the bed-wetting children are *consciously aware* of the causal connection between their bed-wetting and their masturbation (GW-V:236f./SE-VII:74).

If Freud had applied the theory without any qualification—hence also to newly born infants—most readers would have found the theory preposterous. This

circumstance may well constitute the reason why he states that only relapse in bed-wetting after a dry period is caused by masturbation. And this generalization is claimed to have been abundantly verified.

§12. Since Winnicott refers to castration anxiety, I shall supply a childhood recollection from the case-study of the wolf man. During a ride together with his *gouvernante*, the latter's hat blew off. And this event is taken to prove that the child suffered from castration anxiety (GW-XII:42/SE- XVII:19).

There is one sentence I shall repeat ad nauseam throughout the present report. *It is not my fault if authentic descriptions of the content actually found in psychoanalytic writings may sound as if they were evil-minded parodies.*

I could easily write a hundred more pages on the Winnicott quotation (Q-3:1), but will stop here. It is already apparent that Freud's observations are a. exceedingly few; b. exceedingly trivial; c. can be gathered just as easily by any lay man as by a trained psychoanalyst; d. can be gathered just as easily outside as inside the psychoanalytic situation; e. do not provide the slightest support of any psychoanalytic proposition or interpretation; etc.

In a moment we shall see that the spying event in the case-study of Dora is *deliberately faked*.

§13. Lundh's text (Q-3:1) should not go unnoticed. No study has shown that gestalt therapy is of any benefit to any kind of patients. The standard study of psychotherapeutic effect is Rachman & Wilson (1980), but Sjöberg has produced an excellent summary of the situation. A wealth of studies have unanimously shown the effects of all varieties of psychotherapy to be non-existent or microscopic, except studies containing conspicuous methodological flaws. Hence,

"one may safely assume that the results frequently derive from some kind of error of design or other error [...] in the occasional investigations that actually found any effects of psychotherapy." (Sjöberg, 1977:19, transl.) [Q-13:1]

§14. It is now time to turn to *the policy of the open door*. Once more, it may be appropriate to start with the presentation of a series of illustrative examples. All of them consist of statements extracted from Freud's writings, and juxtaposed to form informative patterns. These juxtapositions do not only illustrate the abstract framework of my method—the policy of the open door—but also the concrete procedure that constitutes the corner-stone of my methodology of textual analysis.

## The Seduction Juxtaposition

s-1 *Any hysteric (within Freud's sample of 1896 comprising 18 patients, as well as within the total population of hysterics) will have at least one symptom that is caused by an infantile (seduction) event, and other symptoms (pluralis!) that are NOT caused by any infantile event at all. (GW-I:449, 451/SE-III:212, 214)*



- s-2 *For each and all symptoms of each and all patients of Freud's sample, Freud succeeded in digging out the infantile (seduction) event that had caused this symptom. (GW-I:435/SE-III:199)*
- s-3 *A not indicated number among the 18 patients had (at the age of 2-4) initiated sexual actions involving solely other children and no adult. Freud verified the fact that each and all of these children had previously been seduced by an adult person. And within the pure child relation, they had seduced some other child to perform upon themselves the same variety of sexual action which the adult had performed upon them. (GW-I:452/SE-III:215)*
- s-4 *[The 18 patients consisted of 12 females and 6 males.] A not indicated number of THE BOYS had initiated such sexual actions involving only other children. Freud succeeded in digging out all seductive events there might have been. Nonetheless for some of the boys who had initiated pure child sex he found no previous seduction by any adult. (GW-I:445/SE-III:208)*
- s-5 *For each and all of the 18 patients Freud verified the following sentence. Any symptom will necessarily disappear when the infantile causal event has been dugged out. (GW-I:448/SE-III:211f.)*
- s-6 *The infantile causal event was dugged out for each and all symptoms of each and all 18 patients. (GW-I:435/SE-III:199)*
- s-7 *Some of the 18 patients did NOT lose their symptoms. (GW-I:435/SW-III:199)*
- s-8 *On 21 April 1896 Freud had [according to his own testimony] completed the treatments of all 18 patients and cured all of them of their hysteria. (GW-I:426 et passim/SE-III:191 et passim; Wiener klinische Wochenschrift, IX:20, 14.5.1896, p.1)*
- s-9 *All 18 patients had been treated for a long time, and most of them had had A MINIMUM of 100 hours. (GW-I:458/SE-III:220)*
- s-10 *On 2 November 1895 Freud had not yet met 16 out of these 18 patients. The remaining two were in treatment at this date, but the treatment was not approaching termination. (Schusdek, 1966:159ff.)*
- s-11 *[According to MS:] The period from 3 November 1895 to 21 April 1896 comprises 171 calendar days, both days being included. Let us assume a. that the mean number of treatment hours for the 16 newcomers was 100 hours; b. that Freud worked 6 days a week; c. that Freud had only three additional holidays during Christmas, New Year and Easter (it is known that he had many more holidays); d. that in the lecture delivered on the evening of 21 April 1896 Freud described the situation existing on the same evening. Under this assumptions, Freud devoted a mean number of 11 hours 11 minutes to the 16 newcomers, during each working day.*
- s-12 *[MS:] If Freud had more holidays during the indicated period, he devoted still more hours a day to these patients. Besides, it is improbable that THE MINIMUM for MOST of the 18 patients did not fall short of THE MEAN VALUE for 16 of them. Moreover, it cannot be believed that the information*

*included in the lecture held on 21 April pertained to the situation existing at that date. A much more realistic assumption is that Freud described the state of things existing two weeks earlier. If the latter assumption is taken into account, the mean value will increase to 12 hours 7 minutes a day.*

- s-13 *During the 171 calendar days Freud also treated other patients, viz. the two hysterics whose treatment started before 2 November 1895; and possibly other hysterics whose treatment was not finished on 21 April 1896; and finally all patients with any diagnosis other than hysteria.*
- s-14 *(Unless the total population of patients treated by Freud was highly atypical, the hysterics formed a minority. In the sample collected in 1897 by the first psychoanalyst trained by Freud—to be discussed in the third book—hysterics made up 18%.)*
- s-15 *Freud's statement in s-9 cannot be saved even by a re-interpretation that is generous to an absurd degree. Let "most" patients mean 12 patients, and let these have had 100 hours on the average. Let the remaining 4 patients have had a mean number of 30 hours. These figures will only decrease the time Freud devoted to the 16 patients, to 9 hours 14 minutes a day.*  
[Q-14:1]

§15. According to the theory published in 1895, anxiety neurosis is caused by want of sexual orgasm. In the following juxtaposition Freud describes his alleged observations on his own patients. Note, Freud does *not* use the term "heredity" about genetic properties that are common to all human beings.

## The Heredity Juxtaposition

- h-1 *A hereditary disposition is NEVER missing. There CANNOT be any anxiety neurosis without such a disposition. (GW-I:372/SE-III:137)*
- h-2 *A hereditary disposition alone will NEVER be capable of producing an anxiety neurosis. (GW-I:374/SE-III:138)*
- h-3 *IN MOST CASES a hereditary disposition alone will not be capable of producing an anxiety neurosis. (GW-I:374/SE-III:138)*
- h-4 *IN MOST CASES the specific (sexual) cause will produce an anxiety neurosis ONLY if a hereditary disposition is already present. (GW-I:374/SE-III:138)*
- h-5 *A hereditary disposition is NO INDISPENSABLE CONDITION for the emergence of an anxiety neurosis. (GW-I:374/SE-III:137)*
- h-6 *Freud is inclined to think that ANY HUMAN BEING could develop an anxiety neurosis, regardless of whether he has a hereditary disposition or not. (GW-I:374/SE-III:137)*
- h-7 *In cases in which there is NO INDICATION of a hereditary disposition, sexual factors [i.e., want of orgasm because of abstinence, frigidity, the practice of coitus interruptus etc.] are NEVER absent. (GW-I:371/SE-III:134f.)*

[The last statement clearly implies that Freud has encountered cases in which the anxiety neurosis was caused by sexual factors in the absence of hereditary factors, as well as cases in which the same syndrome was caused by hereditary factors in the absence of sexual factors.]

- h-8 *One will SELDOM find a patient with an anxiety neurosis, for whom a hereditary disposition cannot be detected.* (GW-I:370/SE-III:134)
  - h-9 *There exist a considerable number of women who suffer from A HEREDITARY ANXIETY NEUROSIS. They will develop this syndrome regardless of whatever kind of sexual life they have. Some (or all?) of them suffer from HEREDITARY FRIGIDITY.* (GW-I:364, 374/SE-III:129, 137)
  - h-10 *The specific sexual cause [= want of sexual orgasm] is NEVER missing in any single case.* (GW-I:372/SE-III:136)
  - h-11 *IN MOST CASES the specific sexual cause has been ascertained.* (GW-I:374/ SE-III:137)
- [Q-15:1]

§16. Both the following sets of statements are extracted from the case-study of Dora.

## The Aphonia Juxtaposition

- d-1 *Dora suffered from cough attacks. The latter had a duration of 3–5 weeks, while one attack had a duration of several months.* (GW-V:180/ SE-VII:22)
- d-2 *Dora's cough attacks had a duration of 3–6 weeks.* (GW-V:198/SE-VII:39)
- d-3 *One of Dora's cough attacks had a duration of 6 weeks, and this was apparently an unusually protracted attack.* (GW-V:285/SE-VII:121)
- d-4 *During half the periods of cough attacks, Dora suffered from loss of voice.* (GW-V:180/SE-VII:22)
- d-5 [MS:] *A logically inescapable consequence is that the periods of loss of voice had a duration of something like 1 1/2 to 3 weeks.*
- d-6 [Mr. K. was Dora's father's mistress's husband.] *The periods during which Mr. K. was absent (apparently his business trips) had a duration of 3–6 weeks.* (GW-V:198/SE-VII:39)
- d-7 [Freud explicitly maintains that:] *A necessary consequence of d-4 and d-6 is that: the duration of Mr. K.'s business trips and the periods of Dora's loss of voice, was exactly the same.* (GW-V:198/ SE-VII:39)
- d-8 [Freud explicitly maintains that:] *The similarity of duration described in d-7 proves that Dora was in love with Mr. K.* (GW-V:198f./SE-VII:39)  
[Among all his proofs of Dora's love of Mr. K., the present one is by Freud considered the most powerful. It is the sole proof about which Freud makes it completely clear that the deduction is aimed at proving the very existence of this love, rather than some specific feature of a love the existence of which might perhaps already have been proved by other circumstances.]

- d-9 *During the periods of loss of voice, Dora communicated with her family by writing. She never wrote anything to Mr. K. during the 3–4 years during which she allegedly was in love with him. (GW–V:199/ SE–VII:39f.)*
- d-10 *The short notes on everyday matters written by Dora to her family, were symbolic representations of love letters she would like to write to Mr. K. This interpretation is proved by d-7 together with the following general principle: When the beloved is present, it is appropriate to talk to him; when he is absent, it is appropriate to write to him. (GW–V:199/SE–VII: 40)*  
*The general principle is a part of FREUD’S NOT DORA’S associations to the written notes.*
- d-11 *At first, Freud formulates the hypothesis that there was a perfect coincidence between the periods of Dora’s loss of voice and Mr. K.’s absence. (GW–V:198/SE–VII:39)*
- d-12 *Subsequently, Freud tests this hypothesis on the available clinical observations. He claims that the hypothesis is strongly supported by the data. (GW–V:198/SE–VII:39)*
- d-13 *The empirical support consists of the following circumstance. It SEEMED as if there had, at some not indicated time long ago, been a series comprising a not indicated number of coinciding periods. (GW–V:198/SE–VII:39)*
- d-14 *[According to Freud:] Any instance of non-coincidence had been caused by Dora’s unconscious mind, with the aim of camouflaging the cause of her symptom. (GW–V:198/SE–VII:39)*
- d-15 *[The following statement is by Freud presented as a correct account and a mere repetition of the facts and conclusions of d-11 to d-14] There was a simple and straightforward coincidence between the periods of Dora’s loss of voice and Mr. K.’s absence. (GW–V:212f./SE–VII:52f.)*
- d-16 *[MS:] Freud’s observations of Dora comprised 30 months. During this interval Dora and Mr. K. lived in different towns during two periods comprising about 17 months together. Yet, obviously this absence did not lead to any change in the duration, intensity, or frequency of Dora’s cough attacks or loss of voice. (GW–V:180, 270/SE–VII:22f., 107)*
- d-17 *[Recall from d-15 that Freud verified the coincidence between the periods of Dora’s loss of voice and Mr. K.’s absences. Nonetheless:] Freud claims that it was impossible to establish how often Dora had her cough attacks. (GW–V:180/SE–VII:22)*
- d-18 *[In spite of the claim made in d-17:1] Freud verified the fact that the frequency of Dora’s cough attacks decreased after January 1901 (i.e., shortly after termination of the treatment). (GW–V:285/SE–VII:121)*
- d-19 *When Dora was 14 years old. she was kissed by force by Mr. K. who was some 20–25 years older than her, while she was struggling to make herself free. For any girl the following principle holds: if she does not enjoy a kiss delivered in such a situation, then this fact alone proves that she is a neurotic. (GW–V:186/SE–VII:27f.)*

d-20 *When Dora was 15 years old, Mr. K. tried to seduce her. The fact that she told her parents about the seduction attempt, and this fact alone, proves that she was permeated by a pathological craving for revenge. NO NORMAL GIRL OF 15 WOULD HAVE TOLD HER PARENTS ABOUT A SEDUCTION ATTEMPT.* (GW-V:257/SE-VII:95)  
[Q-16:1]

§17. A critic of psychoanalysis must expect inadequate objections to be raised at each step. Inter alia this one. Freud clearly states that Dora was 16, not 15, years old at the time of the seduction attempt. Hence, MS has obviously read the case-study rather carelessly.

Likewise, Felix Deutsch states that Dora was 42 when he met her—not 40 as MS indicates.

Certain kinds of readers may try to calculate how old a person born on 1 November 1882 was on 30 June 1898 and during autumn 1922.

## The Asthma Juxtaposition

m-1 *Dora developed asthma when she was 8 years old, after having participated in mountain climbing.* (GW-V:179, 243/SE-VII:21, 80)

m-2 *This event of mountain climbing occurred at a time when Dora could hardly walk at all. She had recently hurt her foot through a fall down the stairs. The injury was so severe that Dora still limped at the age of 40.* (GW-V: 243, 266/SE-VII:80, 103)

[Dora's condition at 40 is known from Deutsch (1957).

m-3 [MS:] *Dora's foot symptom is in every respect identical with tendritis with complete degeneration of the tendon. It is sheer nonsense that the symptom was psychogenic and hysterical. Likewise, it is absolutely impossible that this symptom healed up completely at an unstated time during Dora's early teens—as Freud claims—and re-appeared exactly 9 months (= the duration of a period of pregnancy) after the seduction attempt. Freud fabricated this temporal relation in order to prove that Dora made “a false step” at the seduction attempt, in wish if not in deed.]*

m-4 *When Dora was 17 and 18, she was a patient of Freud's for a period of 11 weeks. If Freud told the truth, he did not during this extended period discover that she limped. He detected this symptom during the last or penultimate consultation, and only because of an association to a dream.* (GW-V: 266/SE-VII:103)

m-5 *Dora's asthma started at a time when her father had gone away.* (GW-V: 243/SE-VII:80)

m-6 *The etiology of her asthma was as follows. One night when she was 8 she spied upon her parents having sexual intercourse. Her father who had tuberculosis, was breathing very hard from the effort. Dora was so scared from his breathing, that she stopped masturbating. For this reason she got asthma, which was a masturbatory substitute.* (GW-V:242/SE-VII:79f.)



- m-7 *WHEN FREUD MENTIONS THE SPYING EVENT FOR THE FIRST TIME*, he states that it is no more than *A TENTATIVE HYPOTHESIS* that the event occurred at all. And he explicitly states that his evidence consists of nothing but *INDIRECT SIGNS*. He does not say one words about *WHAT* signs he is referring to. (GW-V:242/SE-VII:79f.)
- m-8 *SIX LINES BELOW*, the spying event has changed into *A PROVEN FACT*. And now Freud states that he has encountered “many” patients whose asthma—as Freud has verified—was caused by spying on sexual intercourse performed by fathers with sick lungs. (GW-V:243/SE-VII:79f.)
- m-9 *AFTER 12 FURTHER LINES*, the spying event has undergone a second metamorphosis: it has been transmuted into *AN OBSERVED DATUM*. Dora herself had *RECOUNTED HER RECOLLECTION* of having spied, having been scared, having stopped masturbating, and immediately afterwards having got asthma. (GW-V:243/SE-VII:79f.)
- [Q-17:1]

§18. These four juxtapositions aptly illustrate the distinctive feature of my approach. *I have placed the door wide open, and have made the reader an eye-witness of everything I do. I shall continually give an explicit description of every single datum I am going to use as a point of departure for any argument. I shall likewise give an explicit description of every single route along which I shall proceed from the data to the conclusion. Moreover, the reader will invariably be told where the original data can be found, so that he can check out the veracity of my account for himself.*

Many of my readers may never have encountered any single writing on a psychoanalytic subject, which went even a few steps in the direction of the policy of the open door. This is true, despite the fact that a limited number of such writings are rather easy to locate. Here, I shall only mention the most important paper ever written on psychoanalysis, Wolpe & Rachman (1960), which has been my main source of inspiration.

I shall add a few trivial qualifications. First, in the survey-like chapters in a later volume, I have drawn heavily upon results obtained by other researchers. Since these researchers have already presented the data bases, one could not legitimately request me to present an exhaustive list of all *their* data.

Second, in some parts of the first book and in certain other volumes, the focus is evidently on formulating and justifying rules for drawing inferences. Many readers might lose the thread, if I continually verified the authenticity of any datum introduced in such sections.

Third, there are things which are so obvious, that there is reason to be suspicious of the motives of any critic who requests documentary evidence. I am not going to prove, nor to invoke any authoritative source in support of, my claim that Reichenberg, the town in which Dora lived in the 1890s, is identical with present-day Liberec in Czechoslovakia.

§19. At the present place I am under no obligation to devote extensive space to the listing and justification of all conclusions I may derive from the four juxtapositions; these things will be done in due course. Some of the conclusions may be

far from obvious, while others may suggest themselves as soon as the relevant statements have been extracted from Freud's writings and compared with each other.

It is immediately seen that the official myth of Freud as a great scientist, who made an abundance of highly unsuspected and very important observations, and who developed new, ingenious and valid ways of drawing inferences from them, has no foundation in reality. Freud's observations are extremely few, extremely trivial, extremely shallow, and extremely atomistic and fragmentary. No behaviourist nor any other scientific psychologist would have dared to publish such things. Freud was extremely incompetent as to clinical observation, empathy, knowledge of human nature, and the logic of inference.

I myself am more interested in certain other aspects. Freud's observation claims are contradictory to such a gigantic degree, that they are empirically and logically impossible: *no human being could ever have made observations even remotely similar to the ones postulated by Freud.*

Most specific criteria for distinguishing *deliberately fabricated or distorted* observations from errors deriving from other sources, will not be presented until in a later volume. Nonetheless, few readers (apart from true believers in psychoanalysis) will have much difficulty in seeing that the statements of the heredity juxtaposition are disseminated by a person who is simply fabulating; who does not in the least describe any clinical observations actually made; and who does not even have any particular patients in mind when he, in a most persuasive way, boasts of all the empirical generalizations he has obtained upon his patients.

*The successive introduction of fabrications in gradual steps* is one of Freud's most recurrent persuasive techniques. If Dora had really recounted the spying event herself, it is incomprehensible why Freud should first present the event as a tentative hypothesis based on indirect signs; and then as a verified hypothesis but still based on indirect signs and not supported by any recollection; and only as the third step as a recollection recounted by Dora herself.

§20. The reader may consider the remaining juxtapositions for himself, or wait until I shall discuss them in detail. At the present point I shall mention a view that is radically different from mine.

I find it rather obvious that the excerpts by Winnicott and Lars-Gunnar Lundh (Q-3:1 and Q-3:2) are typical illustrations of the policy of the closed door, while the four juxtapositions (Q-14:1, Q-15:1, Q-16:1 and Q-17:1) are as typical illustrations of the policy of the open door.

§22. I think I have in the present chapter sufficiently clarified the meaning of the concept named the policy of the open door; have justified the importance of this approach; and have documented that the latter is indeed typical of my report.

One final point. *If two texts contradict each other, and one of them applies the policy of the closed door while the other applies the policy of the open door, the only legitimate decision is to reject the claims of the former text.* To supply an example: Winnicott's postulations in Q-3:1 cannot be maintained in face of, say, the asthma juxtaposition.

Consequently, research based upon the policy of the open door may be more devastating to psychoanalysis, than any other variety of research.

## How to Read Freud: the Jurisprudential Model

*Wer glaubt, gehorcht, des Fragens sich bescheidet.  
Als frommes Rind sein Plätzchen Wiese weidet,  
Dem wird wohl nimmer mit dem Futtergrase  
Die Wahrheit freundlich wachsen vor die Nase.*

Nikolaus Lenau

§23. The advice that one should read this or that book “in a critical way” is at best worthless, unless concrete techniques of critical reading are also taught. An uncritical reader of Q-3:1 may *directly* accept all Winnicott’s dogmatic assertions. A “critical” reader may accept the very same assertions on account of a standard phrase: “These statements sound reasonable, because they have incessantly been asserted by many others. Therefore, I must accept them.”

§24. I shall supply one general rule and describe one concrete technique. As for the former, *anyone wanting to learn about the nature of psychoanalysis, should start to read not the theoretical works but the case-studies, by Freud or his followers.*

This rule is easy to justify. Many of the so-called theoretical works are not theoretical at all. Q-3:1 is typical of this variety of texts. A work is usually called “theoretical”, if it contains *many* empirical generalizations; if the latter are presented *without any empirical backing*; and if they are, as it were, *mentioned rather than described*. The reader may try for himself to extract any concrete empirical generalization from Winnicott’s text: what concrete observations is he referring to? By contrast, the case-studies are replete with concrete illustrative examples of the same nature as the ones included in the four juxtapositions (Q-14:1 etc.), the presentation about G.de B. (§6), the proof of the wolf man’s castration anxiety (§12), etc.

§25. Disregarding the improper use of the term “theoretical”: All sciences are prepared to tolerate greater vagueness or inconsistency at higher theoretical levels. On the other hand, it is much more easy to camouflage a preposterous, illegitimate and altogether unnecessary degree of vagueness or inconsistency at higher levels.

If the reader wants to know whether it is true that “attention cathexes may sometimes be more or less neutralized”, the only choice is to direct an oral question to a proponent of psychoanalysis. The latter will then “explain” the meaning of the sentence by means of another equally incomprehensible word sequence, a

device that may be repeated ad infinitum. The reader may become more and more frustrated. He may often blame himself because of his inability to understand; he may be unable to stand the frustration; and may eventually come to accept some of the odd word sequences as providing a perfectly clear definition.

§26. The technique I shall describe may aptly be named *the jurisprudential model*:

*When reading a psychoanalytic text, the reader should continually perceive the text as a drama enacted in an ordinary courtroom. The reader should place himself in the role of an ideal judge, who is listening to the testimony of the witness and the plea of the barrister, and trying to evaluate both kinds of messages.*

*Freud will sometimes take the role of the witness, and sometimes that of the barrister. The witness Sigmund Freud will supply the fact. The barrister Sigmund Freud will draw the conclusions. It is usually rather obvious whether a certain statement was made by one or the other person.*

The objection that any clear-cut separation of facts and conclusions is alien to Freud's thinking, could not possibly derive from any individual who is familiar with Freud's writings. A less trivial objection would be this. If the technique advocated by MS consists of nothing but separating the facts from the conclusions, the task can be accomplished just as easily *without* dramatizing the text.—But I doubt very much that people who are not professional philosophers will succeed very well without the assistance of a framework which, from the logical point of view, may be redundant.

Moreover, numerous professional psychologists and philosophers (some of whom will be discussed elsewhere), who have proved their impressive virtuosity in separating facts and conclusions whenever the analyzed text is about some subject other than psychoanalysis, have likewise demonstrated their incapacity of applying their own apparatus to psychoanalytic writings.

§27. *The judge must disentangle the real content of the testimony. He must decide whether the witness is trustworthy, and whether the testimony is coherent; whether it is neither impossible nor too improbable; and also whether the degree of concreteness and detail is satisfactory.*

*The judge must peel off the barrister's persuasive techniques and conceptual slips etc. He must establish what conclusions are really warranted by what specific pieces of the testimony.*

I always feel ashamed when praising the jurisprudential model, because real judges who were not in good faith when making their verdicts, may be found in all countries in all ages. But I am talking about an ideal state of things. The jurisprudential model does not differ from any other method in this respect: *no* formal procedure will guarantee appropriate decisions in the absence of a genuine will for truth.

I am very much aware of the fact that the reader will have a tough job identifying and peeling off the persuasive techniques applied in the writings by Freud and his followers. Most readers cannot be expected to succeed without assistance. In a later volume I shall present *a rather exhaustive atlas of Freud's per-*

*suasive techniques*. Some of these will however be described here and there throughout the two volumes that are published now.

§28. It is seen at a glance that all Freud's writings are based upon a simple dichotomy between facts and conclusions. If a thousand psychologists, philosophers, lawyers and journalist were given the task of classifying the statements of the four juxtapositions (Q-14:1 etc.) as to whether which statements were made by the witness or the barrister, few discrepant decisions might be expected, and none that might be important.

The reader should try to classify all the statements of all four juxtapositions. Let us here take a look at the aponia juxtaposition (Q-16:1). It is rather obvious that the witness was responsible for the statements d-1, d-2, d-3, d-4, d-6, d-9, d-17, d-18, the first part of d-19, and the first part of d-20. There might be a slight doubt as to the latter part of d-9. It is rather obvious from Freud's text that Dora did not write to Mr. K., but Freud does not *explicitly* state this. Hence, we *might* conceive of the latter part as the judge's trivial explication of a part of the witness's testimony.

It is as easy to see that the barrister contributed with d-7, d-8, d-10, d-11, d-12, d-14, d-15, the latter part of d-19, and the latter part of d-20.

d-5 is the judge's reflexion—which the reader may here make his own. Of course, the judge's reflexions may be found only in my juxtapositions, not in Freud's original texts.

§29. Only two statements, d-13 and d-16, pose any problems at all. In order to explain how d-16 should be classified, I need refer to a number of additional facts in Freud's paper, which are not clearly present in my juxtaposition. This circumstance will constitute no logical difficulty, since the reader is expected to apply the jurisprudential model to original texts, not to sets of statements extracted from the texts by someone else.

The first half of d-16 is the judge's trivial summary of what is in the most transparent way entailed by the witness's account. The second half is the judge's less trivial conclusion. The witness did not directly say anything as to whether there had been any change or not. But a number of circumstances, which I shall discuss in a later volume, clearly reveal that the witness entertained the view that the change [allegedly!] occurring in January 1901, was the only non-trivial change during those years.

d-13 is difficult to classify. I prefer to attribute the entire statement to the barrister. This means that the barrister's statement is a severely distorted account of what the witness said: the barrister "repeated" what the witness did *not* say.

Some readers may prefer to attribute the latter part of d-13 to the witness. Under this interpretation, the witness was eager to distort the facts, in order to assist the barrister's attempt to prove a position both knew was false. (In the next paragraph I have chosen this alternative.)

§30. I shall now distribute the aponia juxtaposition over the roles.

*The witness testified:*



Dora suffered from cough attacks of a duration of 3–5 weeks, and also of 3–6 weeks, while an attack of 6 weeks exceeded the normal range. During half the period, she lost her voice, and then communicated with her family by writing—but she never wrote anything to Mr. K. during his business trips, which had a duration of 3–6 weeks. It seemed as if the business trips and the aphonic periods had coincided at some indeterminate time in the distant past. Neither the duration, intensity or frequency of the cough attacks changed whether Dora and Mr. K. lived in the same town or in different towns. It was impossible to establish how often the attacks occurred. But their frequency decreased after January 1901.—When Dora was 14 years old, Mr. K. kissed her by force while she was struggling to make herself free. When she was 15 he tried to seduce her. She told her parents about the seduction attempt.

*The barrister deduced the following conclusions from this testimony:*

Mr. K.'s business trips and Dora's aphonic periods had exactly the same duration. This temporal relation proves that Dora was in love with Mr. K. Her love is likewise proved by the fact that there was a straightforward coincidence between both kinds of periods, as the witness has testified. The notes Dora wrote to her family on everyday matters, were symbols of love letters she would like to write to Mr. K. Consequently, they prove that she was in love with him. The fact that she did not enjoy the kiss the much older man forced upon her, proves that she was a neurotic. And the fact that she told her parents about the seduction attempt, proves that she was permeated by a pathological craving for revenge, since it is self-evident that no normal girl would have told her parents about a seduction attempt.

*The judge made the following reflection:*

If both the cough attacks and the business trips had a duration of 3–6 weeks, while the aphonic periods had half this duration, the business trips and the aphonic periods did not have the same duration, nor could they possibly have coincided. [Q -30:1]

§31. In the asthma juxtaposition (Q-17:1) a number of brief insertions are reflexions by the judge: "if Freud told the truth", "when Freud mentions the spying event for the first time", "six lines below" etc. All the remaining italicized text is the witness's contribution.

In Freud's third seduction paper of 1896, 442 words, that is, 4,9% of the text are supplied by the witness. In §243 I shall cite *the totality* of clinical observations contained in this paper.

The reader is now in a position to evaluate a quotation like the following. It should be noted that the two writers, being of an ego-analytic persuasion, specifically apply their praise to the third seduction paper.

"Freud was A CONFIRMED EMPIRICIST. He was also a CAREFUL, KEEN OBSERVER, and some of his work is a delight to read because of his CAREFUL REPORTING OF OBSERVATIONS and THE GENERALIZATIONS DERIVED FROM THEM. It is precisely because of this EMPHASIS ON CAREFUL OBSERVATION that he reformulated his propositions several times, TRYING TO FIND BETTER WAYS TO ACCOUNT FOR their interrelationships. THE DATA WERE NOT IN ERROR. The crucial responses DID OCCUR in his patients. ONE NEED NOT QUESTION HIS STATEMENTS THAT PATIENTS TOLD HIM OF CHILDHOOD SEDUCTIONS AND SEXUAL LONGINGS FOR PARENTS. Rather HIS FORMULATIONS WERE ATTEMPTS TO EXPLAIN THE DATA BETTER.

[...] THE OBSERVATIONS WERE EXTENSIVE. [...]

THERE IS LITTLE QUESTION ABOUT THE BRILLIANCE OF FREUD'S OBSERVATIONAL SKILLS. [...]

He developed his theories out of this MASS OF OBSERVATIONAL DATA. He STARTED WITH DATA, AND INDUCTIVELY DEVELOPED THE THEORY." (Ford & Urban, 1963:148, 174; capitals added) [Q-31:1]

§32. Most people will make minor deviation from truth now and then. Naturally, they expect others to do the same thing. But they cannot imagine that anyone would dare to tell a really gigantic untruth and, in particular, an untruth that is maximally transparent and easy to expose. Consequently, when some cynic really does so, numerous people may think that the statement with which they are confronted, is really true after all. This tendency of great and flagrant untruths being much more easily believed than minor and unobvious ones, will be named "*the principle of paradoxal return*". It is impossible to write an unbiased report on psychoanalysis without referring again and again to this principle.

When one is totally devoid of any supporting data, it is an effective strategy to lay it on extra thick, and boast incessantly of the immense wealth of highly important data one has obtained. Ford & Urban's propagation is just one instance among a thousand others.

§33. B. Farrell has for decades been important as an inventor of a never-ending stream of strategic pseudo-arguments aiming at defending psychoanalysis, cf. §§99–104. In his paper of 1970 he is outrageous at Cioffi's (1970) scientific criticism of psychoanalysis. He implies that it is not legitimate to choose any of Freud's writings as the object of analysis, except the latest and most "mature" ones. At the same time, Farrell considers it perfectly legitimate to *praise* any of the non-late writings because of merits that are flagrantly absent from them; he himself has done so repeatedly.

The point I wish to emphasize is that Freud's writings really become increasingly more "mature" as time goes by—but in a sense that is not very praiseworthy. Neither Freud nor any other psychoanalyst have ever been able to present any non-trivial observations. Nor have they managed to present *both* an interpretation *and* the observations taken as support—without the deduction being easily seen to be invalid.

In his early writings Freud would not infrequently state both his interpretation and the set of observations he used as support. Eventually he learned that this variety of presentation is likely to backfire. *During his life, he turned more and more to the policy of the completely closed door. The witness Sigmund Freud became more and more silent. At the final stage the barrister Sigmund Freud would stand in front of an empty witness box and assert that he had had extensive contact with the witness outside the courtroom; and during these contacts the witness had told him a wealth of factual circumstances. On account of the latter, the barrister's conclusions are firmly verified by the facts.*

The technique of *proving his assertions by the invocation of secret observations* was prominent already in the 1890s, and we shall encounter most amazing

illustrations in the third seduction paper. But this technique became much more pervasive several decades later. Freud ceased altogether to publish case-studies, and the number of observations decreased to a microscopic level.

§34. Highly effective persuaders will at each moment focus exclusively upon the message at hand. They will not in the least bother whether a sizable number of postulations they have made during a few minutes, may contradict each other to a gigantic degree. It must be emphasized that the efficacy of certain techniques depends to a very considerable extent upon what kinds of approaches *listeners* are inclined to permit themselves to be influenced by.

But it is exactly the virtuoso persuader's neglect of any consideration of consistency and coherence, which makes a textual analysis of Freud's writings so profitable. Repeatedly, we may prove that Freud has deliberately distorted or faked his observations. And because Freud does not recall his own untruths from one page to the next, we may sometimes *retrieve the authentic state of things from a highly contradictory set of entirely false statements: there may be one and only one unique state which could possibly have given rise to all the different untruths*. To mention briefly one example that will be extensively analyzed in a later volume. Freud claims on one page that he met Dora for the first time *some two days before* the seduction attempt, and on another page that the same meeting took place *some two months before* the same event. I shall eventually prove from nothing but Freud's published text that the first meeting occurred *about one week after* the seduction attempt. This temporal distortion will turn out to be of utmost significance, because it is a link in Freud's deliberate aim of concealing a fact of which he is perfectly aware, viz. the real cause of Dora's depression.

Some people who are entirely ignorant of the nature of the procedures of textual analysis may, dogmatically and on apriori grounds, declare it absolutely and principally impossible to extract such information from published texts. Interestingly, I have never encountered such persons among experimental psychologists, but only among clinicians accustomed to making wild speculative inferences of a patient's past, on the basis of his present quite different accounts.

§35. However, if the witness box is empty, the task of the textual analyst will be much more difficult. Suppose all the assertions of Q-16:1 = Q-30:1 had been "reduced" to the following vacuous formulation:

"I have observed a wealth of data which conclusively prove that Dora was in love with Mr. K., and that her cough attacks and aphonia were caused by the repression of her love." [Q-35:1]

In that case the textual analyst might not be able to do much more than to point out that the witness Sigmund Freud had kept completely silent.

As time went by, Freud became more "mature", in the sense that he learned more effective strategies of concealing the nature of his thinking. Nonetheless, occasional relapses can be found even in his very latest works. In a later volume we shall take a close look at *Konstruktionen in der Psychoanalyse*, a paper from 1937 which contains many explicit and exhaustive descriptions of an interpreta-

tion as well as those observations postulated to prove the latter. The most important thing to be learned from *Konstruktionen* is not that Freud's thinking was as pseudo-scientific in 1937 as in 1896. It is more illuminating to notice the stagnation and lack of development of his thinking during more than 40 years. There is absolutely no difference between the specific and detailed nature of the procedures of inference, the persuasive tricks, and the deliberate untruths disseminated in both years.

§36. Apart from occasional lapses, the empirical content of Freud's writings became progressively more watered-out. The very same development may be observed within the *International Psychoanalytic Association* during the 5 decades after Freud's death. It is easy to verify the following generalizations: *the greater the empirical content of a psychoanalytic paper, the more transparently absurd is the latter*. Hence, psychoanalytic writings have during 5 decades become less transparently absurd.

It is easy to quote excerpts from Otto Fenichel and Edmund Bergler, two of the most prominent theorists from the 1940s, which few present-day psychoanalysts would dare to defend. Lawrence Kubie and D.W. Winnicott of the 1950s are already much more silent—although they have not yet reached the level of Heinz Hartmann, David Rapaport and Erik H. Erikson of the 1960s. But an unbelievable climax is reached during the 1980s, as exemplified by both a self-analyst and an object relationist, Heinz Kohut (1984): *How Does Psychoanalysis Cure?* and Thomas Ogden (1986): *The Matrix of Mind. Object Relations and the Psychoanalytic Dialogue*.—Except Kohut, all these theorists will be discussed in due course.

Object relationism has become very popular in Sweden during the 1980s. I venture the guess that the reason in this. During the 1960s and 1970s Swedish psychologists were full of hope as to ego-analysis, a hope that was not fulfilled. The *only* merit of object relationism is that it was until very recently an *untried* alternative which, consequently, had *not yet* lead to disappointment.

§37. Winnicott belongs to the object relationists. *On the whole*, he tends to avoid description of concrete empirical circumstances. Nonetheless the most magnificent relapses may be found here and there—even in Q-3:1 in which he thinks that the existence of erections in young male children proves the existence of sexual desire and sexual pleasure. The same fantasy may be found in Haak (1982:83), a former chairman of the Swedish section of IPA who (for reasons I do not understand) considers himself an ego-analyst. Comparing Winnicott and Haak, it seems that the difference between psychoanalysts adhering to different sub-schools may be much less than the difference between practitioners belonging to different generations within the same school.

Anyway, I doubt that anyone would ever be capable of producing a parody of psychoanalysis that would match some of Winnicott's presentations. Babies who have urinated or defecated when lying in bed, may cry. Most people would think the babies cry because of discomfort. But Winnicott informs that this is seldom the case. When the baby feels the dirt against his skin, this sensation will act as a

kind of Pavlovian signal announcing a quite different future unpleasant event. The baby expects to be lifted up and *undressed*, whereby he or she will *lose heat* and *lose the feeling of security*.

“As you know, a baby sometimes cries when he is dirty. This might mean that the baby does not like being dirty (and, of course, if he remains dirty long enough his skin will become chafed and hurt him), but usually it means nothing of the kind—it means that he fears the disturbance he has learned to expect. Experience has shown him that the next few minutes will bring about a failure of all reassurances, that is to say he will be uncovered, and moved, and he will lose heat.” (Winnicott, 5:61) [Q-37:1]

§38. Strange attempts at misusing Pavlov’s theory to fabricate support of psychoanalytic theory, may be found also in Freud’s own writings. Crying babies will usually *cease* to cry when they are taken up and undressed. And numerous babies may lie for hours in dirt, so that they will have little chance of forming a conditioned reflex.

If the problem were worthwhile, we might construct an experimental design for testing Winnicott’s object relationist theory. Let a baby invariably be taken up and undressed three minutes after a bell has rung; and let the dependent variable be whether the baby will start to cry whenever the bell sounds. Or let us raise the temperature so that the baby cannot lose heat when he or she is undressed to remove dirt.

§39. Let us return to Ogden (1986). Any genuine scientist producing a paper with the title indicated in §36 would, as a matter of course, have included numerous authentic psychoanalytic dialogues in the book, and would have taken the latter as the point of departure for his derivations. But Ogden’s book contains a total of 31 literal patient statements and 2 literal analyst statements. There is no single instance of two statements immediately following each other. Fifteen of the patient statements have a maximum length of 3 words, while all of them together comprise a total of 202 words. The analyst statements comprise a total of 71 words.

As if this were not enough, nearly all the quoted statements are so trivial that they might just as well have been skipped. I shall quote one of them:

[P:] It’s too hot in this room; I’m getting out of here. (Ogden, 1986:87) [Q-39:1]

There must be something wrong with people who think they have learned anything from such books, and who see a positive development in the change from Freud’s manifestly invalid deductions to the vacuous products by the most recent psychoanalysts. (When the latter do relapse and give explicit presentations, their deductions are invariably as invalid as those of Freud’s.

One fact should be forgotten. Winnicott is one of the most esteemed theorists among the object relationists of the 1980s.

§40. The reader will easily notice the close relation between the policy of the open door and the jurisprudential model. A researcher cannot easily distribute the content of a psychoanalytic text over the jurisprudential roles, without making the reader an eye-witness of the essential procedures of the research. Nor could a reader easily apply the jurisprudential model to a paper by Freud or any of his

followers, without ascertaining the true content of the paper.—Needless to say, a true believer having no wish to learn the truth, will be capable of applying neither the policy of the open door nor the jurisprudential model, nor any other scientific method of analysis.

The application of the jurisprudential model to the juxtapositions of Q-14:1 etc. may not add any information which could not have been extracted by other means. But this procedure may greatly facilitate the apprehension of information that would be present anyway. Moreover, many readers may find it suitable to start with the application of the jurisprudential model, and let the juxtapositions grow out of this approach.

Already from a glance at the four juxtapositions, the reader will have learned much more about the nature of psychoanalysis, than from Winnicott (1975) and the hundreds of thousands of comparable writings, from which academic course books are usually selected.



# The Multidimensionality of the Concept of Lying

*Da sitzen wir und zerbrechen uns die Kopfe, um ausfindig zu machen, ob er ein Esel oder ein Schurke sei; und am Ende werdet Ihr sehen, dass er Beides ist.*

Christoph Martin Wieland

*A bee or an ant cannot—could not, though he were able to indulge in the pastime of introspection—ever guess that he acted from instinct. Accused of it, he would say, 'Not at all! I am guided entirely by reason.'*

Charles Sanders Peirce

§41. In the present chapter I shall not solve a problem, but merely give some indications as to its complex nature. A number of associated conventional views may merit no comment: that it is a defining characteristic of an academician that he will never cheat in his professional endeavour (a firmly rooted dogm until Broad & Wade, 1983, was published); that it is a sheer impossibility that the acknowledged greatest psychologist of the century has faked his observations; that, regardless of the power of the evidence, normal decency is transgressed by the claim that an academician has faked his data—while it is a perfectly legitimate device to reject the results of a colleague by means of the arbitrary claim that the very fact that he has attacked psychoanalysis proves that he is motivated by nothing but psychopathological blindness.

§42. It must however be stressed that the major part of the research field of lying is still virgin land. I consulted all relevant papers listed in *Psychological Abstracts* since 1927 on the entries “lie” and “lying”. But whenever a paper was of an outstanding quality, it was only marginally related to my subject. Most writers seem to work with a primitive trichotomy: deliberate lying, mistake in good faith, and self-deception that is not under the control of the mind. Generally, lying is taken as a simple phenomenon in no need of any further clarification. And then research is performed, inter alia, on the relation between the lie itself and the context of the lie.

Moreover, all or nearly all instances cited in the literature, of fabrications actually told by various individuals, are so transparent that any lay man can see they are not true. If psychologists and psychiatrists have any specific capacity for exposing fabrications that are not transparent to everyone, they have given little sign of this capacity in their writings.

§43. Very scarce attention has been paid to what may be termed *lie structures*. When the answer is not immediately transparent, how could we detect or prove that a certain statement is not true? or that it is not merely false but *deliberately* false? Such questions have been almost completely neglected. Witness psychologists have made certain non-systematic contributions. With much deference I will however mention Peirre Janet (1893; German tr. 1894; English tr. 1901), a book whose methodology is as satisfactory as might be desired from the most modern view. In the most meticulous way Janet investigated what mechanisms may lead hysterics to make statements that are not in agreement with reality, and he conclusively showed that factors quite different from lying are involved.

§44. Whether or not the concept "lie structure" will in the end be the most appropriate one, few readers may guess what I am after, unless I present a series of concrete examples:

#### *Structure S-A:*

The person P-1 falsely claims to have observed an abundance of the phenomenon F-1. In actual fact F-1 is altogether non-existent in this world. The same is true of any phenomenon even remotely similar to F-1. However, P-1 *honestly believes* that F-1 is a highly frequent and easily observable phenomenon. Hence, to P-1 himself the claim about his own abundant observations may look like a rather trivial departure from the true state of things.

The whole structure consists of one factual mistake in good faith, a full awareness of one factual lie, and one mistake in good faith as to the size of the lie. [Q-44:1]

#### *Structure S-B:*

The first time P-1 claimed to have observed an abundance of the phenomenon F-2, P-1 was perfectly aware of telling a sizable lie. However, over the years he repeated the same claim hundred of times. Eventually he had no feeling of telling anything that was not true. With perfectly honesty he might indignantly rebuke any suggestion that he had told a lie. Nonetheless, if P-1 was somehow forced to pay attention to the question of the truth value of his statement, his original experience of lying would immediately return. [Q-44:2]

#### *Structure S-C:*

P-1 claims to have observed F-3. F-3 is the most bold fabrication P-1 dares assert in one strike. But as a result of his having asserted F-3, his *adaptation level* will change, that is, his perception as to what kind of lies are petty, middle-sized, and big. Hence, P-1 will now dare assert the greater lie F-4. The latter will once more change his adaption level, and he will eventually assert also F-5. [Q-44:3]

### *Structure S-D:*

P-1 starts out with the aim of inducing the reader to believe F-5. But he realizes that almost any reader would perceive F-5 as altogether incredible, if this fabrication were introduced in one step. Hence, P-1 cuts F-5 into three progressively more encompassing "slices", F-3, F-4 and F-5, which are presented in the order of increasing empirical content.

A typical example may be the three versions of the spying event, cf. Q-17:1. When the first version is introduced, the reader will raise his eye-brows. He may however calm down when he realizes that the spying event was claimed to be no more than a tentative hypothesis.

When Freud thereafter advances the next claim that the spying event is a proven fact, the reader has already become somewhat accustomed to the idea itself: his adaptation level has changed. He will again raise his eye-brows, but not as much as he would have done without the preparation through the first version.—The same sequence will be repeated at the introduction of the complete version, viz. that Dora herself *recalled* having spied, been scared, and stopped masturbating. [Q-44:4]

### *Structure S-E:*

The functioning of the mind of P-2 may aptly be described in terms of the structure of an organization. Incoming post will first go to a department which will determine whether the received message is offensive to the organization. If it is not considered offensive, it will proceed to a department which will determine whether the message is true. Then it will go to the board for a decision as to the needed external action.—However, if the first department finds the message offensive, it will go directly to the board. And the question whether the message is true, will never be asked.

P-2 may be a psychotherapist. The patient may remind him of earlier statements of his. The tape-recorder may unambiguously have registered these statements only a few minutes ago. But P-2 may exclusively pay attention to one single aspect: if he had said such things, he would have said something silly (apparently he is functioning at a lower mental level when he makes his statements, than when he evaluates them in retrospect). He may aggressively shout that it is just the patient's pathological fantasy that he ever said this.—Would it be appropriate to say that P-2 is *lying*? [Q-44:5]

§45. I have given only a few specimens of what may turn out to be a very comprehensive set of highly unexpected structures. At the present time no one seems to have tried to produce such a set. When this task has been solved, the trichotomy of lying, self-deception and mistakes may be impossible to retain. Meanwhile we have no choice but to apply this trichotomy, although we shall do well to make some subdivisions.

Throughout Freud's writings there are an abundance of instances belonging to each of the three categories. It goes without saying that it may be difficult or impossible to decide to which category certain instances belong.

Errors deriving from carelessness are so extremely frequent, that it is difficult to understand how anyone could praise Freud's writings because of their "crystalline clarity" (Miller, 1973:500).

Self-deception definitely constitutes the only reasonable explanation of the humiliating interpretations aggressively fired at Dora during the last 7 pages of chapter III of this case-study. When Dora stated her decision to quit immediately, Freud got a nervous break-down. He felt *a severe disappointment and a craving for revenge*. He was altogether unaware of the fact that it was he himself who had these feelings. He mechanically imputed them upon Dora. Nowhere in psychoanalytic literature can a more convincing description of the phenomenon of *projection* be found, than on these 7 pages. (To prevent a misunderstanding I hasten to add that the observable part of the mechanism of projection was known at least 100 years before Freud was born.)

§46. Numerous psychiatrists have read Freud's writings or have met him face to face at congresses. Many of these had no vested interest in psychoanalysis. Nonetheless, they did not detect that Freud was incessantly telling deliberate untruths.

Some devices for defending Freud are particularly astonishing. Edward Glover invented the phrase "inexact interpretations" about downright false interpretations. A representative of the British school of psychoanalysis might well plagiarize this idea: if the authentic facts are that *no* patient recovered in a sample of 18 hysterics, then Freud's claim that *all* 18 patients were cured, is merely *an inexact way of describing the true state of things*.

§47. I shall now compare the structure S-C and S-D. And the comparison will turn out to have methodological and theoretical implications.

It may safely be assumed that the reader's reactions will be *the same* in both structures: his adaptation level will change in the same way and because of the same causal factors. By contrast, S-D entails that Freud was *fully aware* of what he was doing, and that he intentionally played on a certain mechanism in the reader. According to S-C, Freud was instead *the victim* of his own changed adaptation level. Although he knew that he had not told the truth, he had no awareness of the size of his fabrications.

Is there any objective procedure for distinguishing between S-C and S-D? Can we establish, with certainty or with a high degree of probability, that the pattern around the spying event belongs to S-C but not to S-D, or vice versa? The answer is affirmative.

§48. Freud invariably borrowed ideas which were already popular in his culture and age. Like so many others he imitated thermodynamics in a most primitive and mechanical way. Sexuality may be transformed into neurotic symptoms in the same way in which electricity is transformed into light. In both cases the amount of energy before and after the transformation is exactly the same.

Consequently, children will never develop any neurotic symptoms as long as they continue to masturbate. In 1896 and 1905 Freud imagined that children will usually masturbate until the age of 8. But at that time masturbation will stop abruptly as the result of one single frightening event. Freud's justification for this prominence of the age of 8 was presented in §9.

§49. In other words, Dora must have masturbated until she was 8, and must suddenly have stopped. We are supplied with 8 masturbation proofs:

*Proof no. 1:* Dora suffered from white vaginal discharge. And in Freud's view this ailment is in young girls primarily an indication of [childhood] masturbation. (GW-V:238/SE-VII:76).

This proof is easily seen to be based upon *the principle of similarity*: vaginal fluid is similar to male semen; hence, it must have a similar cause.

*Proof no.2:* Dora wet the bed when she was 7–8. And as far as Freud knows, bed-wetting after a dry period has no more likely cause than masturbation. In Freud's experience, bed-wetting children are *consciously aware* of the connection between their bed-wetting and their masturbation. (GW-V:236f./SE-VII:74)

Once more the proof is based upon *the principle of similarity*: bed-wetting is similar to nocturnal seminal emissions (GW-V:90/SE-VII:190).

In view of the abundance of fabricated observations throughout Freud's writings (cf. also proof no.5), there is no reason to believe that Dora was ever a bed-wetter. (But nothing hinges upon this point.) Bed-wetting during childhood is a symptom which may with little risk be imputed upon any individual.

*Proof no.3:* Dora dreamed about a fire. Freud's *not Dora's* association to this dream is that one will often say to children: if they play with fire they will wet the bed. Hence, the fire dream proves bed-wetting; and bed-wetting proves masturbation. (GW-V:233ff./SE-VII:71ff.)

At the start of the treatment Freud made an initial anamnesis. But if he told the truth, he completely forgot to inquire whether Dora had wet the bed, despite the fact that he knew in advance that bed-wetting is a valid indicator of masturbation, while he also knew that masturbation was a crucial factor behind Dora's symptoms. Thereby he did not destroy the possibility of illustrating the power of the psychoanalytic method in making-correct inferences of childhood events from dreams.

*Proof no.4:* Dora had stomach-gripes. And it is a *well-known* fact that stomach-gripes are very frequent among masturbators. (GW-V:241/SE-VII:78f.)

*Proof no.5:* Dora thought there was a causal connection between her disease and her father's syphilis. Freud's argument is not made altogether explicit. But only one interpretation will make sense. Both syphilis and masturbation are "shameful

sexual diseases". Hence, when Dora postulated a causal relation between her symptoms and her father's shameful sexual disease, she really meant to assert a causal connection between her symptoms and her own shameful sexual disease. (GW-V:237f./SE-VII:75f.)

Without any attempt at evidential support at the present place, I shall mention the fact that it is a deliberate fabrication that Dora's father ever had syphilis; that Dora knew or believed that he had so; and that Dora saw any causal connection between her symptoms and her father's syphilis.

*Proof no.6:* Dora had undergone numerous medical treatments over the years. Some of these must have been very painful, and none of them had been of any help. Eventually she refused to see any further doctors. She submitted to be treated by Freud solely because she did not dare oppose her father's stern command.—I shall take this information at face-value, despite the fact that there is hard evidence to the effect that Dora did *not* refuse to see *other* doctors than Freud.

Freud ignores all the natural motives Dora could have had. Without any evidence he points out her "real" motive: she was afraid that the doctors might discover that she had masturbated. (GW-V:241/SE-VII:78). They would have inferred masturbation from the vaginal fluid.—But *what* doctor apart from Freud would ever have arrived at such an idea?

*Proof no.7:* During a consultation Dora was playing with her handbag. (I surmise it was a birthday present she had just received. In 1900 buckle locks seem to have been a fascinating innovation.) Dora repeatedly and absent-mindedly put her finger into her handbag, closed the latter, and pulled the finger out. This behaviour is said to prove a. that Dora had masturbated during childhood, and b. that she would like to masturbate now. (GW-V:238f./SE-VII:76)

Just for the sake of the argument, let us assume that this deduction is in principle correct. Then what different behaviours would Dora have manifested, if she had had the present desire but not the past experience, or vice versa?

*Proof no.8:* One day Dora was sitting in the waiting room reading a letter. When Freud called, she hurriedly concealed the letter, although its content "was a matter of complete indifference and had no relation to the treatment". Dora's behaviour proves that the letter represented her childhood masturbation, the secret she wished to conceal from Freud. (GW-V:240/SE-VII:78)

Later, Freud would ardently deny the very possibility of events having no relation to the treatment. And whatever the content of the letter, it would have been a matter of routine for Freud to deduce any unconscious phenomenon from it.

Moreover, the present case-study contains an unusually large number of highly significant letters, inter alia a suicide letter. Besides, Dora is said to have tried to conceal not only masturbation but also homosexuality, father incest, and love of her father's mistress's husband.



Nonetheless, the parsimonious explanation is that Dora did not try to conceal the letter. When the doctor calls, many patients will hurry up, feeling that they are wasting the doctor's time if laying away a letter takes as much as a quarter of a minute. Throughout *Gesammelte Werke* Freud repeatedly proves his lack of empathy and observational skill. [Q-49:1]

§50. Still today, most psychoanalysts think that Freud really proved that Dora had masturbated. The list of the above 8 proofs will give a deeper understanding of psychoanalytic observations, methods and theories, than most of the extensive books on these subjects. But this is not the point I am going to make now.

Look at the pages on which the proofs are found. In *GW*: 233ff.—236f.—237f.—238—238f.—240f.—241—241. The three versions of the spying event are presented on pp. 242f.

Flagrantly, Freud is eager to prove that Dora masturbated around the age of 8. But I feel unable to imagine that Freud would have taken himself so much trouble of constructing so many pseudo-proofs, all of them based upon nothing but indirect signs, if he had known when he wrote pp. 233–241, that he would on p. 243 state that *Dora herself had recalled* that she had masturbated at 8. The only reasonable conclusion is that Freud knew nothing of any such recollection, until he arrived at p. 243—which means that we have here a further and independent proof that the spying event is fabricated.

To sum up: if the pattern of the three versions of the spying event is compared with the 8 preceding masturbation proofs, the more encompassing pattern is compatible with S–C but not with S–D.

§51. I think there are other instances in *Gesammelte Werke* which are solely compatible with S–D. At the present place I shall say nothing about these.

A brief anticipation. In volume II I shall say much about the relation between Freud's personality and his lying behaviour. My central idea is this. Freud had—probably since childhood—been accustomed to saying (e.g.) “the chair is red”, not when the chair was in fact red, but when something could be gained by uttering this statement.

§52. All varieties of research involve taking risks. And since the researcher himself may be blamed for any poor results deriving from an inappropriate choice, the researcher himself should decide what kinds of risks *he* is prepared to take. People with scarce knowledge of the history of science may again and again construct philosophical proofs to the effect that this or that problem cannot profitably be attacked, until this or that problem is solved. For instance, the concept of the lie must be clarified by semantic analysis and/or psychological experimentation, before historical research based upon the method of textual analysis has a reasonable chance of being successful.

I suggest that things are the other way around. Much historical research must be performed, before we may hope to arrive at a sufficiently sophisticated grasp of the field to conduct adequate and informative experiments and semantic analyses. Those disagreeing with my suggestion are welcome to try out other ap-

proaches, but not to prescribe how I should proceed. How could anyone create an experimental design or a semantic technique which would have any chance of discovering the above listed lie structure, unless he had already observed them in a non-experimental or "non-semantic" context?

§53. Throughout *Gesammelte Werke*, the number of errors deriving from carelessness are truly gigantic. Flagrantly, Freud was functioning at an exceedingly low level of attention when he conducted his therapy and wrote his papers. His attention and mental effort reached a normal or high level solely when his was concerned with persuasive techniques.

He had very confused ideas as to Dora's age at different events, including the most crucial events, as well as her age when he treated her. In the third seduction paper he claimed that 18 patients out of 20 makes 80% (GW-I:435/SE-III:199f.). Listing all such oddities would be boring. However, certain errors of carelessness may disclose unexpected information about Freud himself.

§54. The following facts from the case-study of the rat man are taken from Freud's case-notes, which were not published until 1974. Some of the most crucial information is missing in both GW and SE. During the second consultation this patient described an oriental variety of punishment. A pot of rats was tied to the victim's posterior, so that the rats would gradually gnaw away the buttocks. In the beginning Freud misunderstood the account and thought the patient was talking of impalement. For all we know about Freud, the idea of impalement could hardly fail to evoke the idea of coitus per rectum.

But even when the mistake had been corrected, Freud seem to have suffered from *perseveration*. If the pale was thought to enter the anus and to symbolize a penis, then the rats must likewise enter the anus and symbolize a penis.

If this hypothesis is correct, *one of the most central interpretations of the case-study of the rat man did not derive from psychoanalytic theory, but from a primitive psychological mechanism outside Freud's control.*

§55. In the case-notes (Freud, 1974:34-44) Freud makes it clear *how* he can know that the patient fancied about rats entering the anus. Freud had in advance established that the patient was homosexual. His proof must be cited:

1. When the patient was 4-5 years old, he had a very young and very beautiful gouvernante named *Miss Robert*; "Robert" was her family name.
2. One evening he was permitted to touch her sex organ. He himself took the initiative, he was not seduced.
3. "Robert" is usually a boy's first name.
4. At the age of 29 the rat man did not recall Miss Robert's first name. This is in Freud's view a remarkable fact BECAUSE a gouvernante is USUALLY called by her first name.
5. At 29 the patient was not surprised that he did not recall the first name.

[Q-55:1]

In turn, the rat man's homosexuality is said to prove that he was unconsciously thinking of rats creeping into the anus and symbolizing a penis.

§56. One cannot say that here is a conclusive proof of the phenomenon of perseveration. Nonetheless, the probability is strengthened by the fact that additional examples may be found, which are as inconclusive in themselves, but which are *isomorphic* with each other.

On 31 December 1900 Dora announced her decision to quit the treatment immediately. She said that she had entertained this decision for some weeks. Freud became furious and ad hoc fabricated a flood of humiliating interpretations which he heaped upon her. I shall supply only one illustration. Dora is said to be *treating Freud like a gouvernante, giving him notice with two weeks*. Evidently, Dora did not at all give Freud notice. ("14 days" is the German phrase Freud iterates.) We have already encountered Freud's habit of proving causal relations by fabricating temporal relations (cf. Q-16:1). It is improbable that Dora ever said that she had entertained her decision for 14 days. But Freud fabricates that *14 days* is a recurrent feature in Dora's thought: she told her mother about the seduction attempt *14 days* after the event. And then the words are put into Dora's mouth that she told her mother on *14 July*, and that the seduction attempt took place on *30 June*. Allegedly, Freud commented during the consultation: "Again a fortnight then—the time characteristic for a person in service" (GW-V:270/SE-VII:107).

§57. In a later volume I shall prove that it is a deliberate fabrication that Dora told her mother about the seduction attempt before she told her father. In actual fact she told her father immediately, after the event, hence about one week before homecoming.

Although the proof is once more not conclusive, the most parsimonious explanation is that the date of 14 July was again fabricated solely because the idea of giving notice with 14 days was continually on Freud's mind.

(Until I present the comprehensive analysis of the case-study of Dora, I shall often take at face-value the information that the seduction attempt took place on 30 June 1898, that is, at a time when Dora was 15 years 8 months old.)

§58. A third example. During Dora's railway station dream she repeatedly asked: "Where is the [railway] station?" (GW-V:258/SE-VII:94). A few sentences will be quoted as to Dora's associations and Freud's reflexions upon the latter. I shall freely add and delete italics:

[Dora was looking for a certain postcard.] "It had been put in a *box* for keeping pictures in [Bilderschachtel], and she could not lay her hands on it at once. She had therefore said to her mother: 'Where is the *box*?' [Schachtel]" (GW-V:268/SE-VII:95f.)

"The relation of the question in the dream to the question which had been put in real life *allows us* to substitute "*box*" [Schachtel] for 'station'. A *box* and a woman: the notions begin to agree better." (GW-V:259/SE-VII:97)

"'Where is the *key*?' seems to me to be the masculine counterpart to the question 'Where is the *box*?' [Schachtel]. They are therefore questions referring to—the *genitals*." (GW-V:260/SE-VII:97) [Q-58:1]

To Freud, a box or a letter-case would automatically suggest the female sex organ. In fact, "Schachtel" was common slang when Freud grew up (Kleinpaul, 1885:167).

§59. Throughout the entire case-study Freud incessantly asserts that Dora was in love with Mr. K. We have in Q-16:1 seen Freud's utmost strongest proof of Dora's love. But *at only one place* does he assert the reciprocal interpretation that Mr. K. was in love with Dora, viz. during the last 7 pages of chapter III. There is not the slightest attempt at any proof. Freud felt a severe disappointment when Dora decided to quit. He projected this emotion upon Dora, and constructed a fantastic interpretation. When Dora told her parents about Mr. K.'s attempt at seducing her, she entertained the secret hope that her father would talk to his friend, and the latter would then admit his feelings toward Dora, whereafter both males would agree on engagement between Dora and Mr. K., after the latter had divorced his wife. When Mr. K. denied everything and blamed a teenager's highly-strung fantasy, Dora allegedly felt "a severe disappointment". Freud thinks that Mr. K.'s contemptuous attitude toward Dora—which he retained for some 34 months—is perfectly compatible with his being deeply in love with Dora and wanting to marry her.

§60. As for Mr. K.'s love, it can hardly be doubted that Freud originally felt that the main text supplied perfectly satisfactory evidence. He *might* have given a second thought to the problem, but this is unlikely. However, in his letter of 3.3.1901 to Fliess, Freud recounts that he had let "Oscar" read the case-study "but I derived little joy from it." Perhaps Oscar pointed out the absence of anything even remotely akin to a proof of Mr. K.'s love. At any rate, Freud added a footnote containing what he must then have felt as a satisfactory proof:

"In particular there was a speech which he had made in presenting Dora with a letter-case [Briefschachtel] for Christmas in the last year in which they lived together at B-." (GW-V:271/SE-VII:108) [Q-60 :1]

§61. This brief footnote merits many comments, in addition to the preceding remark. *It is one of Freud's habitual techniques to prove his interpretations and theories by invoking secret evidence.* And this technique is by no means original: acknowledged pseudo-scientists as well as lay men who are aware of not having any evidence, regularly do the same thing.

I doubt very much that Dora would have accepted a gift from Mr. K. only 6 month after the seduction attempt (granted that it took place on 30 July), while Mr. K. still stuck to the idea of Dora's highly-strung imagination. Much information about Freud's lack of knowledge of human nature is revealed by the fact that he is unable to perceive any inconsistency of Mr. K.'s behaviour.

Furthermore: *I challenge all psychoanalysts to try to construct a speech which Mr. K. might have made, and which would prove his love.*

§62. But now comes the point I shall make. It can hardly be doubted that Mr. K.'s speech is a deliberate fabrication. But it seems that this fabrication involved a letter-box (Briefschachtel) simply because the concept of a box/Schachtel was

incessantly on Freud's mind when he wrote chapter III, as well as when he re-read this section to improve the evidence.

§63. I have provided three *isomorphic* examples. When seen in isolation, none of them can be said to contain conclusive proof that *perseveration* was responsible for the interpretations. When taken together (and many more analogous examples may be found throughout *Gesammelte Werke*), it would be a far-fetched hypothesis that each example merely *happened* to agree with the pattern of perseveration.

While psychoanalysts have always claimed to be free from reality-distorting mechanisms; to know themselves to a degree unexcelled by any other group of scientists; and to have the most comprehensive knowledge of the functioning of the human mind ever possessed by any group of psychologists; the truth is that they are completely ignorant of the functioning of the human mind, and of the existence and nature of reality-distorting mechanism, and that their self-knowledge is flagrantly deficient. Many psychoanalytic interpretations which have been accepted for nearly a century, seem to derive from the most primitive and sub-rational mechanisms.

§64. Without any attempt at proof until in a later volume, I shall suggest one more aspect of carelessness as the cause of widely accepted theories and interpretations.

It is rather easy to see that the chapters of the case-study of Dora that were written in this order: III–II–I–IV; and that the last 7 pages of chapter III were the very first section to be produced. Chapter III is about Dora's railway station dream, while chapter II is about the fire dream. The railway station dream was discussed during the three last consultations. I shall in due course prove that it is a sheer fabrication that Dora ever claimed to have dreamed the fire dream at the time of the seduction attempt. Freud was perfectly aware of the fact that the content of the fire dream was *causally unrelated* to the seduction attempt. But in order to fabricate a *causal relation* he applied his habitual technique of fabricating a *temporal relation*.

§66. He also states that the fire dream occurred during the treatment, "only a few weeks" before the railway station dream. This temporal indication is probably a mistake in good faith.—There is no hard evidence as to the date of the dream. But a plausible hypothesis is that long-distance guests had arrived to celebrate Dora's 18-year-birthday on 1st November. Her mother may have planned to place an extra bed in front of the door to her brother's room. But her father had absolutely forbidden this solution because the brother would then have little possibility of escaping in case of a fire; the door might have opened outwards. There was a quarrel between the parents, and the fire dream was according to Freud *released* but not *produced* by this quarrel.

If this hypothesis be true, the fire dream preceded the railway station dream by some 8 weeks. Of course, the family might have had long-distance guests also at other occasions. However, the reader may try to imagine for himself how the mother might have blocked the door at night only, in other ways than by placing a bed next to it.



§66. James Strachey has produced no less than two different translations of the case-study of Dora, which have entered *Collected Papers* and the *Standard Edition*, respectively. Hence, he had ample opportunity to correct any error. I am nevertheless prepared to excuse the linguistic part of his flaw: "absperren" means "to block", not "to lock". But Strachey evidently has little knowledge of human nature, since he is capable of imagining Dora's mother being in the habit of locking in each night a 19-year-old boy who would only three years later become a famous labour leader. Allegedly, the father had always accepted this habit as a matter of course. But for no reason at all he suddenly did not want this state of thing to go on any longer, started a quarrel, and referred to the son's situation case of a fire. (GW-V:226/SE-VII:65)

What I have said now should not be construed as a comment on Strachey alone. The very fact that such a pattern has been disseminated in the two most widespread editions of Freud's writings, without anyone being startled, reveals the low degree of empathy of the entire psychoanalytic profession.

§67. Freud's claim that both dreams in the case-study were written down immediately, cannot be taken seriously. Freud explicitly states that dream analysis must not take as prominent a place in every psychoanalytic treatment, as it did in this case. However, the entire case-study contains a total of two (2) dreams. If Dora dreamed a total of two dreams during 11 weeks (an assumption that is not highly probable), Freud's just cited statement must be a deliberate untruth. If, on the other hand, this statement is true, there must be a number of additional dreams which Freud had *not* written down immediately, and which he had completely forgotten when he produced the case-study. Moreover, a number of features of his descriptions seem to indicate that the circumstances around the fire dream had been stored in Freud's memory for quite a long time, before they were written down.

§68. Things stored in memory are prone to undergo certain kinds of changes. I shall quote a lucid description of the latter:

"The story becomes progressively more logical and more concise. Irrelevant details and inconsistent elements drop out. Familiar activities like fishing [in a concrete example the writer had just presented] are substituted for the more unfamiliar, hunting seals. And the story conforms more to the kinds of cause-effect relationships with which we are familiar." (McKeachie & Doyle, 1966:317) [Q-68:1]

To apply these rules to Freud, they must be so construed that details and elements which are irrelevant and inconsistent *from the psychoanalytic point of view* will drop out, while the sequence of events will come to conform to the kinds of cause-effect relationships familiar to *psychoanalytic theory*.

If the hypothesis I shall present is true, Freud was a *victim* of distortion rather than the *agent* of distortion.

§69. The railway station dream is a potpourri of incoherent and unrelated fragments. The fire dream is a well-structured and logical short-story. Admittedly, many dreams have a coherent and logical structure. Hence, we are not entitled



to conclude that Dora could not have dreamed the fire dream as it is quoted in Freud's paper. Turning however to the pattern of subsequent associations, exactly the same features are repeated. The pattern surrounding the railway station dream is a chaotic potpourri containing an abundance of details which from the psychoanalytic point of view are of little interest, and which Freud does not use to throw light upon anything. Freud apologizes because of this character of his account. But I would rather say that his account gives the impression of being in the main authentic and trustworthy.

The pattern surrounding the fire dream consists of a veritable flood of Freud's own associations based upon *the principle of similarity*. For instance, if Dora's parents had an argument about a pair of ear-drops, this proves that Dora wanted a few drops of semen from Mr. K. (GW-V:230, 252ff./SE-VII:68, 90ff.) But when seen in a different perspective, the pattern is exceedingly clear. There are very few details that are irrelevant from the psychoanalytic standpoint. Everything is, as it were, "put in order", "prepared" for the psychoanalytic explanation.

§70. The railway station dream with the surrounding pattern was definitely written down a few days after Dora recounted it (the last three consultations were devoted to the analysis of this dream). The surrounding pattern of the fire dream gives strong indications of having been thoroughly transformed while being stored in Freud's memory for no brief time (and probably for about 8 weeks). The fire dream itself may or may not have been transformed in the same way.

A hypothesis which easily suggests itself is this. If Freud had written down the fire dream and its pattern immediately, at least the pattern and possibly also the dream itself might have constituted a confused, incoherent and ill-thought-through potpourri, which would include an abundance of details about which Freud would not have known what to do. If Freud had stored the railway station dream and its surrounding pattern in his memory for some two months until he wrote them down, both the dream and the pattern might have had the character of a well-structured short-story and a well-prepared body of evidence more or less inviting a psychoanalytic explanation.

§71. In other words, I surmise that *the difference between the surrounding pattern of the two dreams in the case-study of Dora, and possibly also the difference between the dreams themselves, may correspond rather well to the difference between the form Freud's observations usually had when he made them, and the form they had when they had been stored in Freud's memory for some time*. Numerous "observations" in Freud's writings may be non-authentic because of circumstances which bear only a remote relation to lying.

I myself adhere to Karl Jaspers's (1958:559) suggestion that "recht lügen können nur die ganz Wahrhaftigen". To use the lie as a specific tool without being influenced by it in any deeper sense, is a less objectionable behaviour than to be so permeated with mendacity that one is not even aware of lying.

§72. Strictly speaking, we are not now concerned with lying. Nonetheless, the degree to which an individual is a victim of the non-rational mechanisms I have

outlined, is to some extent the individual's own responsibility. He may for decades have *indulged* in such mechanisms, and may have fancied himself to be such an infallible truth-finder that he is in no need of any precaution. Although all human beings are afflicted with the same kinds of mechanisms, some researchers are aware of their weaknesses, try incessantly to keep them in check, to circumvent them (rather than to subdue them), to "deceive the deceptions".

I have not in this volume proved my surmise in §71. Nonetheless, the reader may greatly profit from the warning that, apart from the deliberately fabricated or distorted data, many other observations in *Gesammelte Werke* may be highly dissimilar to what Freud actually saw with his eyes or heard with his ears.

§73. Lastly, I will present an instance which cannot be classified with certainty, but which may nonetheless throw much light upon Freud's cognitive apparatus.

In the case-study of Dora we may read:

[Dora] "told me that one day she had met Herr K. in the street while she was walking with a cousin of hers who did not know him. The other girl had exclaimed all at once: 'Why, Dora, what's wrong with you? You've gone as white as a sheet!' She herself had felt nothing of this change of colour; but I explained to her that the expression of emotion and the play of features obey the unconscious rather than the conscious, and are a means of betraying the former." (GW-V:219/SE-VII:59) [Q-73:1]

Incredible as it may seem, Q-73:1 is presented as a proof of Dora's love of Mr. K. But while there exists such a thing as the paleness of love, Freud must be the only person who has taken "being white as a sheet" as evidence of love. There are many more odd points. The description is wanting in several important details. How did Dora and Mr. K. react? What explanation of her paleness did Dora give? Had she felt scared or repulsed? It is a normal human reaction not to be *reflexively aware* of one's physiological condition, if one's attention is completely absorbed by an uncomfortable stimulus. Did Dora still feel no physiological change after her attention had been drawn to her whiteness? Besides, the vegetative reaction "obeys" the *spontaneous conscious* (whether she felt love or anxiety or repulsion, these were conscious emotions); and not the *unconscious*. Evidently, Freud had no grasp of the distinction between the spontaneous consciousness and the reflexive consciousness. Hence, he was in no position to decide whether the assumption of the unconscious was needed to explain any phenomenon.

§74. *It is one of Freud's habitual techniques to put his fabrications into the mouths of other people.* The context leaves no doubt that Dora's cousin took her whiteness as a proof of her love. But it is improbable in the extreme that two individuals would arrive at exactly the same far-fetched mistake, and would then come to interpret the same behaviour of the same person.

§75. *There is a surprising similarity between the event with the cousin, and an event in Nicholas Nickleby by Charles Dickens.* Previously, Sir Mulberry Hawk had attempted something intermediate between a seduction and a rape of Kate Nickleby. Kate's mother is depicted as a comical figure, who is as proud of her alleged ability of perceiving the real meaning of other people's behaviours, as

she is wanting in any such ability. Not long after the sexual attack Kate meets Sir Mulberry at the theater in company with her mother:

[Kate's] "surprise was so great, and, moreover, brought back so forcibly the recollection of what had passed at Ralph's delectable dinner, that she turned extremely pale, and appeared greatly agitated, which symptoms, being observed by Mrs. Nickleby, were at once set down by that acute lady as being caused and occasioned by violent love. [...] She was in no small degree delighted by this discovery, which reflected so much credit on her own quickness of perception [...]" (Dickens, 1982:177, ch.27) [Q-75:1]

To my knowledge, no researcher of literature has ever suggested that Kate's paleness proves her love for Sir Mulberry. The idea that Freud, Dora's cousin, and Mrs. Nickleby would independently of each other make exactly the same mistake, cannot be taken seriously.

§76. There is no room for any doubt: Q-73:1 is modelled after Q-75:1. The only question is how Dickens' text came to enter Freud's paper. There are several possibilities.

Freud might have read *Nicholas Nickleby*, and may deliberately have imitated the scene in this novel, after having deliberately transformed a mother to a cousin. But while instances can be found of Freud applying this technique, I doubt very much that he would have taken the risk of imitating a text which might directly invite the comparison between Freud himself and Mrs. Nickleby.

Freud might however have had only a vague recollection of the novel and the scene, and might not have recalled what girl had become white as a sheet—could it have been Dora?

§77. Not necessarily the true hypothesis but apparently the most probable one, is this. Dora had really met Mr. K. and become white as a sheet. She may well have been alone on the occasion, but at least no follower had taken her whiteness as a sign of love. Freud had then delivered the dogmatic interpretation that the whiteness constituted a valid proof of her deep positive feeling. *Dora* had however read *Nicholas Nickleby*.

And she had used Dickens' description in self-defense. During the consultations she had (in respectful formulations) emphasized the analogy between Sir Mulberry and Mr. K., between Kate and herself, between both seduction attempts, between the paleness of both girls, and not least: between Mrs. Nickleby's and Freud's interpretation of this paleness. In addition to the analogies, she had stressed Dickens' evaluation of Mrs. Nickleby's interpretation—an evaluation which must be equally appropriate if applied to Freud.

But when Freud later was writing the case-study, he vaguely recalled Dora becoming pale at the sight of Mr. K.—and some female relative who took the paleness of some girl as a sign of love. Being unable to recall who the relative was, he deliberately decided to make her a cousin.

§78. There are additional plausible routes from Dickens' novel to Freud's case-study—some of them differing little and others differing much from the discussed ones. The reader may try to find a few of them for himself. As for the last

hypothesis—which I think is in essence the correct one—only minor modifications are needed to delete any trace of deliberate fabrications; *or*, for maximizing the part deriving from deliberate lying. I see no prospect of disclosing what set of modifications will come closest to the real state of things.

§79. Anyway, the central problem of *my* report is Freud's *deliberate fabrications and distortions*. I am under no obligation of investigating also his self-deception and carelessness, or instances which cannot be classified in any unambiguous way. Nonetheless, I have shown that the study of Freud's carelessness may yield unexpected and highly interesting results.

Moreover, I have asserted my view that an adequate concept of lying will have to be multidimensional. No such concept is in view, and the search for it has hardly begun. We have hardly the faintest ideas as to what *could* be a dimension.

But! there is no ground for the expectation that Freud would appear in a more favourable light, when an adequate concept has been developed.

## The Method of Textual Analysis Compared With the Method of Historical Documentation

*Vnd woher will man sich vergewissern / das auch eben der Calculus Brahaeus so gar just seye. Er hat ja eben so wol als andere vor jhme kein andere Demonstrationes, als die Geometrische Instrumente / mit welchen sie jhre observationes motuum stellarum verrichten. Wer ist aber jemahlen in Himmel hienauff gestigen / zuerfahren / ob sie gar eygentlich zutreffen?*

Philippus Feselius

§80. I am the first researcher in Sweden who has applied the method of textual analysis within psychology. And I know of no other international researchers who have applied the same approach except Joseph Wolpe and Stanley Rachman. The method of textual analysis is a non-experimental technique. Some people might therefore expect opposition to arise from experimentalists, but not from clinicians accustomed to applying non-experimental procedures. As could easily have been predicted, quite the opposite is true. Experimentalists who have had no personal experience of applying anything even remotely similar to my techniques, have unanimously acknowledged the validity of my fundamental approach, as well as the basic validity of my results. Moreover, they have found no difficulty in pointing out occasional errors. This circumstance would seem to suggest that the technique of textual analysis is in excellent agreement with accepted scientific methodology.

By contrast, clinicians of a psychodynamic persuasion agree that the method of textual analysis is not scientific, and that the method could not possibly yield any valid result, but will merely supply subjective prejudices in disguise.

§81. Their animosity is easy to understand. Until now, such clinicians have been free to disseminate the most incredible postulations about the wealth of observations they have made in the consultation room: secret observations which provide conclusive proof of psychoanalytic theory. They have been accustomed to reject the most firmly established experimental results with a stroke of a hand: if experimentalists cannot find any support of, say, the idea that asthma is caused by spying upon fathers with sick lungs practicing sexual intercourse, then this circumstance merely proves the limited power of the experimental method.

If the clinical field is conquered by scientific methodology, hardly any refuge

will be open to such clinicians. Hence, the introduction of valid procedure into their own area will constitute a much greater danger than any negative facts obtained elsewhere. Recall what a hard blow it was to psychoanalysis when effective therapeutic techniques "intruded" upon a field they had so far monopolized.

§82. I am strongly indebted to Layzer (1972) for the clarification of my thinking. Within the entire field of methodology no rule can be found that is more dear to me, and to which I adhere more obstinately, than this one.

*It is a maximally false view that:*

*When we conduct experimental research, we need submit to strict rules. But when we leave the laboratory and turn to "real life situations"—to which, oddly as it may seem, the psychotherapeutic situation is reckoned—no rules need be obeyed. Here, soft procedures are not only legitimate but are the only appropriate ones.*

*I take the very opposite position. Within any kind of research the same two distinctions are mandatory. Veracious observations must be distinguished from imaginary impressions. And well-founded inferences must be distinguished from idle speculation. [Q-82:1]*

§83. *I emphatically reject the suggestion that the methodology of textual analysis is to the least extent in need of any kind of external validation.* Specific arguments might be needed to justify my specific applications, but this is not relevant since my critics have rejected the method in toto and per se.

Suppose backward time travel became a reality. In that case we should no doubt learn numerous new facts about Napoleon, Thomas Aquinas, The Black Death, and so on. But what should we think of an individual who suggested: at the present time it is merely a speculative hypothesis that Napoleon was defeated at Waterloo; that Thomas Aquinas was a Dominican; and that millions of people were killed by The Black Death around the middle of the 14th century. But after time travel we would know that these things really happened.

It would likewise be a curious idea that, until a human being had visited the moon, we did not know that absence of atmosphere, an extreme difference between day and night temperature and a very weak gravitational power, are characteristic of this celestial body. Or that it is at the present time merely a speculative hypothesis that there is no organic life on the sun.

Whether related to physical, behavioural or historical sciences, such ideas are neither more nor less foolish than Feseliuss' (1609:B recta) objection that Copernican astronomers have no evidence for their heliocentric theory, since none of them has ascended to heaven in order to observe whether their theory is true.

In the same way, a statistician would feel perplexed at the suggestion that his inferences from random samples are pure guess work: until the entire population has been studied, he does not at all know whether pulmonary cancer is more frequent among smokers or non-smokers, nor whether the blood-group AB- is particularly infrequent in Sweden.

§84. I have tried to publish three previous versions of my investigation on the seduction theory. Two of these were based on nothing whatever but textual



analyses of Freud's published text (the third seduction paper), since the relevant historical documents had not yet been published, or had been published so recently that I had had no time to glance even casually at them. Having eventually scrutinized also the historical documentation, I can easily demonstrate the almost perfect agreement between the results obtained both by methods. These results are listed in Table 84:1, in so far as they are related to the sample of April of 18 patients.

Flagrantly, each and all the 11 results are highly surprising and most daring. These are not the kind of hypotheses one would have any chance of constructing on the basis of guesswork or personal prejudices (granted that they are true). Note that 10 out of 11 results which were obtained by means of nothing but a textual analysis of Freud's published text, have been completely verified by the available historical documents. As I said, I reject the idea that these results were in need of any kind of external validation. But I very much doubt that my critics, so courageous about rejecting a method which has within psychology (but not within history, linguistics, and many other sciences) been applied by only one researcher in this country, would dare to reject Freud's own testimony in his own private letters, according to which none of the 18 patients were cured, and none of them had recalled any repressed event etc.

It must be a curious method which, on the one hand, is invalid and, on the other hand, will yield almost nothing but results known in independent ways to be valid.

§85. The only statement in Table 84:1 which has not been confirmed by historical documentation, is item no. 10. Obviously, we could not expect two highly different approaches to yield *exactly* the same results. There must be innumera- bly many circumstances that can *only* be digged out by means of the method of textual analysis, or *only* by means of historical documentation.

In Table 84:1 I have presented the 10th statement in the strong form I have defended during 1984–1988. In the present version of the report I have softened the statement, since competent and benevolent critics have doubted the power of my deductions.

§86. My less competent and less benevolent critics may hope to retain their original position by means of *one and only one auxiliary hypothesis*, viz. that I am bluffing. I myself claim to have arrived at each and all the results listed in the left column of Table 84:1, before I had the slightest idea of the existence of the historical documents. But according to the auxiliary hypothesis—which may be named “the bluff argument”—MS had actually studied the historical documents carefully before he started to perform the so-called textual analysis. From these documents MS had learned what results might safely be fabricated. Afterwards MS constructed pseudo-deductions for the purpose of feigning that the results had been deduced from Freud's published text.

In an earlier version of the report I have exposed all the difficulties of the bluff argument. I have decided to delete the entire section, except for a few comments, believing that most readers would find the section rather boring.

Table 84:1.

RESULTS OBTAINED BY TEXTUAL ANALYSIS	RESULTS OBTAINED BY HISTORICAL DOCUMENTATION
1. <i>Freud observed no instance of de-repression.</i>	<i>Freud observed no instance of de-repression.</i>
2. <i>Freud observed no instance of sexual seduction which was repressed.</i>	<i>Freud observed no instance of sexual seduction which was repressed.</i>
3. <i>Freud observed no instance of sexual seduction which was causally relevant to the emergence of the patient's symptoms.</i>	<i>Freud observed no instance of sexual seduction which was causally relevant to the emergence of the patient's symptoms.</i>
4. <i>Freud observed no instance of sexual seduction at all.</i>	<i>Freud observed no instance of sexual seduction at all.</i>
5. <i>Freud did not cure any of the patients by de-repression of any seduction event.</i>	<i>Freud did not cure any of the patients by de-repression of any seduction event.</i>
6. <i>A number of patients whom Freud claims to have cured, were not cured at all.</i>	<i>Freud definitely did not cure any patient at all.</i>
7. <i>Freud almost certainly did not cure any patient at all.</i>	<i>Freud definitely did not cure any patient at all.</i>
8. <i>Not only Freud's published observations about each patient were extremely shallow, trivial, and non-psychoanalytic. His secret observations can, by means of valid inferences, be proved to be of exactly the same nature.</i>	<i>Not only Freud's published observations about each patient were extremely shallow, trivial, and non-psychoanalytic. The observations stated in documents not intended for publication, are of exactly the same nature.</i>
9. <i>Not only Freud's published observations about each patient were extremely scarce, fragmentary, and atomistic. His secret observations can, by means of valid inferences, be proved to be of exactly the same nature.</i>	<i>Not only Freud's published observations about each patient were extremely scarce, fragmentary, and atomistic. The observations stated in documents not intended for publication, are of exactly the same nature.</i>
10. <i>As for 14 out of the 18 alleged patients, Freud did not have any particular individuals in mind. In other words, these patients were fabricated.</i>	<i>There is no indication as to whether Freud may or may not have fabricated any patients who actually did not exist.</i>
11. <i>In sum, Freud had faked nearly 100% of his claims, and fully 100% of all his non-trivial claims.</i>	<i>In sum, Freud has faked nearly 100% of his claims.</i>

I would welcome the increased vigilance of the academic community as to the possibility of bluffing—if it be a *general* attitude. So far, only people aiming at defending Freud at all costs, have accused MS, and only MS, of bluffing (cf. §21).

The bluff hypothesis would be less unlikely under the assumption that MS entertained the secret aim of propagating the superiority of the method of textual analysis, while MS cared much less about the nature and position of psychoanalysis.

§87. Interestingly, semi-public papers distributed in 40 copies exist as far back as 1977—when few people knew anything about Jeffrey Masson, Alice Miller, and the Fliess letters—in which I have accused Freud of having deliberately distorted the observations in the case-study of the rat man. Other available semi-public documents prove that I have asserted all the statements in the left column of Table 84:1 not later than 49 days after the date of the publication of Masson (1984), in which many excerpts from the crucial Fliess letters are quoted. His book was not available in Sweden at the same date. And I do not think anyone would suspect me of being so hyper-intelligent, that I would have managed in less than 49 days 1. to read Masson's book, 2. to read Freud's third seduction paper, 3. to fabricate the proper statements, and 4. to construct numerous pseudo-arguments which would have a reasonable chance of being mistaken for valid derivations. (And note, neither Masson nor any of his thousands of readers have suggested that any of Freud's observations are faked.)

It is easily seen that there is no room for any explanation based on self-deception: the hypothesis based on the suggestion that MS is deliberately dishonest, is the only alternative to accepting the results of Table 84:1 as proving that the method of textual analysis is valid.

It would be interesting see my critics elaborate the concrete details of their own implicit hypothesis, e.g. in relation to the seduction juxtaposition (Q-14:1). If the juxtaposition does not provide any support for any of the statements listed in Table 84:1, what parts of Masson's book inspired me to extract exactly these statements from the seduction paper? And how did I dare to assert that none of the 18 patients were cured, since *this* information cannot be found in any of the excerpts from the Fliess letters included in Masson's book? Freud's testimony on this point was not published until about a year after my semi-public assertion.

§88. From the logical point of view the attempts at disproving the validity of my methodology merit no comment. I might have made occasional blunders. Or the method might turn out to be in need of minor modifications. All scientific methods are imperfect in the beginning, mathematical statistics not excluded. Such imperfections can only be remedied by future research. It is to a considerable extent an empirical discovery that a certain method will yield results which should be accepted.

## The Significance of Strategic Pseudo-Arguments

*Habe ich jemals in meinem Leben gelogen?*

Adolf Hitler

§89. It will eventually become clear from my report that no published or secret observation has ever been collected, which may provide support for any psychoanalytic proposition or interpretation—if we disregard interpretations which the psychoanalysts themselves consider “shallow”, and which were well-known long before Freud was born. Then how did psychoanalysis become recognized as a legitimate, and perhaps even a true, scientific theory? Doubtless, one of the main factors is the propaganda with which this theory has been promoted. In the present chapter I shall give only a few brief remarks on this subject. In a later volume a little more will be said.

§90. Far from all behavioural scientists are aware of the fact that persuasive and suggestive techniques may be quite different from iterated hammering of the persuasive message. Many clear-cut persuasive stratagems may at first glance look like a neutral presentation of firmly established empirical research results or valid logical deductions. Scharnberg (1984, ch.4) listed 7 methodological techniques for fabricating “evidence” even in the laboratory—where there is none. All 7 procedures are based upon *parasitism*.

In the natural raw situation numerous causal relations may be temporally correlated. The event C-1 (the cause) may be responsible for the event E-1 (the effect). Likewise, C-2 may be causally responsible for E-2. But we may entertain the erroneous hypothesis that C-2 is responsible for E-1. Since C-1 and C-2, respectively, as well as E-1 and E-2, may be temporally correlated, it may be a matter of routine to “verify” our false hypothesis.

§91. Parasitism may be associated with both *generalization to properties* and with *generalizations to individuals*. Behavioural scientists are more likely to overlook flaws related to the former aspect. In so far, the former aspect is the more important. To save space I shall however provide an illustration related to the latter. Suppose I want to prove the astrological theory that people born in Leo are particularly prone to suffer from heart diseases. I may simply compare two samples of patients: Leo-individuals at a clinic for cardiovascular diseases and non-Leo-individuals from an orthopedic clinic.

With at most one exception—Halpern (1977)—each and all experiments listed in Hilgard (1952), Fischer & Greenberg (1977) and Kline (1972, 1981)

have applied designs based on parasitism upon *non-psychoanalytic* causal relations, and they do not differ in any non-trivial aspect from the just described astrological design. Eysenck & Wilson (1973) have thoroughly re-analyzed those 18 experiments to which Kline attributes a particularly strong evidential power. An additional set of experiments praised by Fischer & Greenberg (1977) have been re-analyzed in Scharnberg (1984), and a number of further experiments will be examined in a later volume of the present work. Hence, there is no reason to say more about this issue now.

Numerous psychoanalytic papers as well as sections on psychoanalysis in textbooks in general psychology, will contain numerous instances of techniques of intermediary types. Freud's own papers feign to be based upon rational derivations.

§92. Persuasive devices looking like scientific arguments may be applied within many sectors, inter alia the following: 1. empirical research reports; 2. other academic writings including textbooks; 3. popular science literature and educational materials; 4. disseminations in mass media, whether produced by ignorant journalists or academicians or both in collaboration; 5. expert pronouncements influencing the distributions of grants by research funds; 6. oral arguments applied in academic lectures and at seminars. As for the last sector, I am concerned with arguments advanced by teachers not by students.

There is no hard boundary between the varieties of arguments found within these sectors. Some arguments may be frequent within all sectors. But a certain argument may 30 years ago have been commonplace in many papers by the most prominent researchers of the field, while 15 years it was encountered only in less known papers by less known researcher; and today the argument may only have survived in oral communication. Alternatively, an argument may be tested orally upon different audiences during a decade, before it appears in print.

§93. It is my hypothesis that whatever happens within *any* of the listed sectors (and others not listed) may have severe consequences for the future development of the entire discipline. Even *oral* arguments which have never appeared in print may, if they are widespread and incessantly propagated and never rebuked, become a fundamental constituent of the standard for evaluating any new contribution to the field. They may encourage certain kinds of poor research, and intimidate objective research on certain problems. They may motivate persons in key positions to stop certain uncomfortable reports.

Hence, it may be important to study the prevailing arguments found within *each* of the sectors. For obvious reason I cannot devote any extended space to a subject falling outside the scope of my report. Nonetheless, I will discuss a few pseudo-arguments. One argument is widespread over the whole world, despite the fact that reflexion for a few minutes may suffice for seeing that it is altogether invalid: the idea that any flaw in Freud's writings derives from his aim of protecting the anonymity or privacy of his patients. This argument cannot be ignored and the following chapter will be devoted to the analysis of it.

§94. It has *always* been the strategy of *acknowledged* pseudo-scientists to re-



buke objective research results by the claim that the researcher has proved nothing. Instead of having investigated the matter he has merely tried to pass his prejudiced attitude for the outcome of an empirical investigation. Martin Gardner (1981) has quoted an impressive number of criticisms by occultists who have put on the fancy-dress costume of the meticulous methodologist.

The same technique is repeatedly applied by proponents of psychoanalysis within *all* the listed sectors. But while there is little difference between the proponents of psychoanalysis and occultism there is an enormous difference in the reactions of the academic community upon them.

§95. Although the number of *widespread and highly successful* strategic pseudo-arguments is excessive I shall in the present chapter focus upon only one specific argument, and one series of arguments invented by the same writer: B. Farrell. The specific argument is the idea that only someone who has undergone a personal psychoanalysis is competent of criticizing psychoanalysis.

Evidently only a pseudo-scientist could advance the idea that only psychoanalyzed individuals are capable of perceiving whether there are any contradictions between the statements in the four juxtapositions Q-14:1, Q-15:1, Q-16:1 and Q-17:1. Equally evident, this fact has no *psychological* power since the academic community has accepted the psychoanalysts' right to rebuke any variety of criticism with the argument that the critic has not undergone a personal psychoanalysis. (I refuse to supply any autobiographic information as to whether I have been psychoanalyzed.)

§96. Instead I shall suggest that psychoanalysts using this argument are perfectly aware of not meaning what they say. To my very considerable knowledge *no* proponent of psychoanalysis has ever taken the position that the non-psychoanalyzed critic is incompetent, while the psychoanalyzed critic is competent. Instead, psychoanalysts unanimously agree that neither psychoanalyzed nor non-psychoanalyzed critics may legitimately criticize the theory. The only difference is that criticism by a non-psychoanalyzed writer will be rejected on the ground that he has not been psychoanalyzed, while criticism by a psychoanalyzed writer will be rejected on the ground that he has been psychoanalyzed. This personal experience of the latter is said to have produced a "negative transference", whence he is destined to perceive flagrantly true statements as false, and flagrantly false statements as true. (This argument is so widespread, that there is no need to supply any references.)

The *only* way of the latter critic of becoming "competent", is to undergo further psychoanalysis, until he has "realized" that there are no contradictions between the statements of the four juxtapositions. When he has realized this, he is competent to criticize psychoanalysis.

§97. I do agree that psychoanalytic treatment is an effective tool for achieving this kind of cognitive change. The demand for a personal analysis is nothing but a strategic pseudo-argument to certify that only true believers will be permitted to criticize the theory.

The very same psychoanalyst may not infrequently claim that psychoanalytic



treatment could not possibly produce any kind of harmful effects—and that it may for time and eternity cause a highly gifted and responsible researcher to produce nothing but downright absurd research results.

§98. There is a long tradition of misusing philosophy for the purpose of confusing the issue, so that one's favourite theory may be retained despite a sizable number of hard facts. The strategic pseudo-arguments of the Padua school against Copernican astronomy have been described by Pierre Duhem (1969). And while statistics reveals that a negro slave imported from Africa to the United States would on the average live 10 years before he was worked to death (Hobhouse, 1985), Lady Victoria Welby constructed a specific philosophic method by means of which she "proved" that the opponents of slavery had committed a semantic blunder and contaminated two things that are in actual fact quite different: being a slave, and living an uncomfortable life (Hardwick, 1977). In other words, Jürgen Habermas, Paul Ricoeur and Jacques Lacan had many predecessors.

§99. However, apart from the hermeneutics, few proponents of psychoanalysis have produced so many strategic pseudo-arguments as B. Farrell, or have had so much success with them. Pseudo-arguments are often "worn out" after having been in constant use during a number of years. But when his own previous ideas are replaced with new ones, Farrell does not care whether his different positions are internally or externally consistent. The only constant feature is the purpose of undermining the basis for criticism.

His primary idea may aptly be called *the argument of writer's cramp*. Farrell (1970) finds it difficult to deny the nature of Freud's writings. Whenever Freud states both an interpretation and the evidence claimed to prove it, the evidence invariably consists of a few trivial observations which flagrantly prove nothing. There is no trace of the alleged wealth of observations upon which each interpretation is allegedly based. Farrell never supplies any concrete examples, but a typical one would be the statements d-2, d-4, d-6, d-7 and d-8 in the aphonia juxtaposition (Q-16:1): the fact that both the aphonic periods and Mr. K.'s business trips had a duration of 3–6 weeks, conclusively proves that Dora was in love with Mr. K.

Farrell dogmatically asserts that Freud's theory and methodology was the very opposite of the ones stated in his writings [including the writings never intended for publication]. Allegedly, Freud was perfectly aware of the fact that all his published observations are trivial and prove nothing. But he actually derived Dora's love from the [*fabricated*] temporal relation *together with thousands of additional observations not included in the published paper*. And if the latter are taken into account, Freud's inference (interpretation) will appear much more well-founded. Freud just happened to suffer from an attack of a psychic variety of writer's cramp each and all times he sat down at the desk, so that he completely forgot the nature of the methodology he invariably applied in the consultation room, as well as the theory he adhered to at all other times.

§100. Farrell wisely abstains from any attempts at explaining 1. how he could

possibly know that Freud entertained a quite different theory and applied a quite different methodology; 2. any feature of this secret theory and methodology; 3. how he could possibly know that the latter are of a much more scientific nature; 4. why each and all Freud's followers suffer from the same variety of writer's cramp whenever they sit down at the desk, since they must be assumed to apply Freud's satisfactory theory and methodology in the consultation room; 5. why his followers unanimously praise Freud's writings because of their "crystalline clarity and profundity" (D.R. Miller, 1973:500) and the wealth of carefully reported observations (Ford & Urban, 1963, cf. Q-31:1) as well as the unexcelled "consistency and rigor" of his inferences (Eissler, 1965:57); 6. why his followers unanimously attribute psychopathological mechanisms to anyone who cannot find any trace of these features in Freud's writings; 7. how Farrell's construction could possibly be applied to Freud's interpretations of behaviours and traits shown by historical persons, on the basis of their autobiographies or similar published documents; 8. how the construction could possibly be applied to the numerous instances in which Freud states that he does not know anything about the individual except the single isolated observation upon which his interpretation is based; etc.

§101. As I have already said, *the persuasive techniques* of Freud and his followers will be presented and analyzed in a later volume. But it must be said at this point that Farrell's construction is a typical *one-step argument*: the construction may seem plausible enough, as long as one takes only one step along the argument. But as soon as one takes a few further steps, the argument will collapse by its own weight.

§102. Six years later Farrell (1976) put on the fancy-dress costume of the meticulous methodologist and pointed out the ambiguity of the concept of an interpretation. Because of this problem, the search for criteria was said to be "so difficult and frustrating" (Farrell, 1976:31). But Farrell deliberately confuses two issues. It is by no means difficult to suggest criteria by means of which Erich von Däniken's theories can be *tested*. But it would be a most "frustrating" task to search for criteria by means of which they can be *verified*.

§103. One more standpoint is expressed by Farrell another ten years later. Grünbaum (1984, part I, ch. 2) does not only show that the truth of psychoanalytic interpretations was in the beginning proved from nothing but their therapeutic effect—while they allegedly remained proved when the psychoanalysts later denied the therapeutic effect of their treatment. Grünbaum also points out that psychoanalysts have not yet substituted the abandoned criterion with any other warrant. No new warrant is even in view. Hence, *neither Freud himself nor present-day psychoanalysts have any reason for believing that their interpretations and old or recent theories are true*.

The following is Farrell's answer. Present-day psychoanalysts might admit that Grünbaum's criticism is correct as far as it goes. But they might retort that his criticism is not relevant, because

“analysts are not especially interested in Freud’s original defence of his method. They are concerned with the defence of present-day psychoanalytic theory and work.” (Farrell, 1986:237) [Q-103:1]

§104. I note in passing that *exactly* this technique of confusing the issue is commonplace in the psychoanalytic consultation room. Moreover, Farrell’s last idea is closely related to the fourth proposition in the chapter on recent development of psychoanalysis.

But whatever we may think about Farrell himself: *the academic community must accept its share of responsibility for having accepted such pseudo-arguments and, hence, for having encouraged the habit of constructing and disseminating them.*

I shall finally repeat the fact that the arguments actually presented in chs. 5 and 6 are no more than typical specimens of a comprehensive set of strategic pseudo-arguments, which have been incessantly applied by psychoanalysts at least since 1896.

## Did Freud Try to Protect the Anonymity and Privacy of his Patients?

*There are some things which cannot be burlesqued, for the simple reason that in themselves they are so extravagant and grotesque that nothing is left for burlesque to take hold on.*

Mark Twain

§105. Apart from the idea that a non-psychoanalyzed individual is incompetent of criticizing psychoanalysis, no strategic pseudo-argument is as widespread as the idea that *any* flaw pointed out in Freud's writing may be given the highly probable and perfectly natural explanation, that it derived from Freud's aim of protecting the anonymity or privacy of his patients.

The first seven statements of the seduction juxtaposition are flagrantly contradictory. The juxtaposition may however be partitioned into two non-contradictory subsets consisting of, on the one hand, s-1, s-3, s-5, s-6 and, on the other hand, s-2, s-4, s-6, s-7. If the anonymity hypothesis is spelled out completely, it will take the following form. Freud might have included only the second subset into his paper. But if he had done so, many readers would have learned *from this subset alone* that, say, Bishop August Sömmering of Innsbruck and Judge Joachim Zerbst of the Supreme Court were two of the 18 patients who had been sexually seduced during pre-school age. But if *the very same four statements* be included *together with four additional statements*, there is no possibility of extracting the identity of the Bishop and the Judge from the text.

§106. If the privacy hypothesis is completely spelled out, it will take an analogous form. If the bishop and the judge had read only the second subset in Freud's paper, they would have felt that Freud had exposed in public too many intimate details about their private life. By contrast, if the very same four statements had been included together with three additional statements, the bishop and the judge would have felt that Freud's text did not contain any intimate and private information.

A highly excentric logic or semantics are needed to make sense of such arguments. But both arguments entails one more oddity. Freud's "modification of the authentic facts" must not only protect anonymity and privacy better than the non-modified version would have done. In addition, the modified and the non-modified facts must be equally compatible with the theory, and must have more or less the same degree of evidential force, as the authentic data.

§107. The utmost central principle of psychoanalytic theory is expressed by Freud himself in the following words:

“Every time we come upon a symptom we can infer that there are certain definite unconscious processes in the patient which contain the sense of the symptom. But it is also necessary for that sense to be unconscious in order that the symptom can come about. Symptoms are never constructed from conscious processes; as soon as the unconscious processes concerned have become conscious, the symptom must disappear.” (GW–XI:288f./SE–16:279) [Q-107:1]

But now we are told that the principle that *all* symptoms will disappear as soon as *all* unconscious phenomena have become conscious, is perfectly compatible with the claim that *some* symptoms did *not* disappear even though *all* unconscious phenomena had become conscious.

The reader may try for himself to analyze the remaining three juxtapositions in the same way.

§108. These strategic pseudo-arguments may be refuted in a quite different way. Is it true that Freud’s papers contain no conspicuous cues to his patients identity—cues whose inclusion into the paper cannot possibly be justified by any psychiatric considerations? Is it true that highly intimate details of the patients private life have been deleted from Freud’s published papers?

I shall focus upon the case-study of Dora. Note, many of the proponents of the two pseudo-arguments are known to have been perfectly aware of the facts included in this paper.

Dora is said 1. to be in love with a man who was some 20–25 years older than her, and to wish to suck his penis; 2. to be homosexual; 3. to desire to sleep with her father; 4. to have masturbated at the age of 8, and to desire to masturbate at the time of the treatment; 5. to feel a pathological craving for revenge.

It is easily seen that attributions such as these would around the turn of the century compromise Dora very severely—while it would have be of no consequence to her reputation whether the periods of loss of voice had a duration of 3 to 6, or 1 1/2 to 3 weeks. Moreover, I shall prove in a later volume that Freud was perfectly aware of the fact that Dora was not in the least homosexual, nor was she in love with the man or with her father.

§109. Now to the cues to her identity. She suffered from cough attacks of some 3 to 6 weeks, which were associated with complete aphonia. Such a symptom is impossible to conceal from neighbours, fellow students, servants, distant relatives etc. Many gossip mongers who had a neighbour with this symptom might wonder whether this neighbour might be identical with Freud’s patient. Evidently, it would be a strategic pseudo-argument that a case-study described in a medical periodical would have only a neglectible chance of becoming known to Dora’s lay acquaintances.

Freud’s paper contains an abundance of information 1. whose inclusion in the paper cannot be justified by any pschiatric considerations whatever; 2. which flagrantly is seen to be of such a nature that it should have been deleted at all costs;

and 3. which could most easily have been deleted or modified without any non-trivial distortion of the clinical picture.

In fact, *the view might be defended that Freud, when he published all these cues, had a specific aim: to ensure that Dora would be recognized by many people—in particular by those whose knowledge of her identity would make her feel most ashamed.*

I shall discuss this view in a later volume.

§110. First, we are told that Dora was 18 years old when Freud treated her in 1900. Hence, she was born in 1882.

Second, the three towns in which she lived are indicated. Vienna is explicitly named. Merano is indicated as a health-resort in the Southern part of Austria, which was suitable for persons suffering from tuberculosis. Readers over the entire North-Western and North-Eastern Europe would immediately realize what town Freud was talking of.

Third, we are told that Dora's father was the owner of a factory. And long before I learned about Dora's identity from other sources, I guessed from certain details in Freud's text that he had a *textile* factory. Reichenberg (= present-day Liberec in Czechoslovakia) is indicated as a manufacturing town other than Vienna. Throughout the entire Double Monarchy the first two towns to suggest themselves would be Reichenberg and Budapest. And there is no hint of Dora having lived in an area dominated by some language other than German. In the 1890s there were 144 textile factories in Reichenberg, employing about 139 000 workers (carpet factories and many other textile-like enterprises are not included in these figures). A number of other industrial branches were of a comparable size. Moreover, Freud indicates the capital of Böhmen (Bohemia) by the letter "B".

§111. This body of information does not *prove* that the third town was Reichenberg. But it is sufficient that Freud's indications may stimulate a *guess* which actually is correct. As soon as a gossip monger has guessed the truth, many ways will be open for verifying the guess. Did Dora's father have tuberculosis? Did he have a second residence in Merano? Did Dora have a brother who was about one year older, and no further siblings? etc.

§112. But now comes the most revealing—and, from the psychiatric point of view, least important—information. *Freud exposed the years and annual seasons of Dora's five movements between the three indicated towns.*

§113. The central problem is not that psychoanalysts—just like other pseudo-scientists—may say *anything* to save their favourite theory. The essential aspect is that the academic community is so prone to accept all varieties of strategic pseudo-arguments "supporting" psychoanalysis, even if their logical structure is of such a kind that they would not even be considered worthy of refuting in any other context.



## Checking Citations

*Carl-Erik Brattemo, a professor of psychology and a practicing psychoanalyst, wrote a whole page in a newspaper, in which he assured the reader that he had carefully compared all accounts of Freud's case-study of Dora in Scharnberg's doctoral thesis, with Freud's original text. Thereby, it had become clear that these accounts consist of nothing but dishonest, propagandistic distortions. Scharnberg should be compared to a Tibetan juggler.*

*Professor Brattemo's carefully verified result is so much the more astonishing, as the case-study of Dora is nowhere mentioned in Scharnberg's doctoral thesis.*

*Scharnberg presents very few direct quotations from Freud's writings. Hence, one is not sure whether Scharnberg's accounts are really correct. In one instance I have compared Freud's original text with Scharnberg's account. Freud states that it was Katharina's uncle who seduced her cousin and also showed some interest in Katharina herself; according to Scharnberg's account, Katharina's father was however the one who seduced both her sister and herself.*

Lars Gunnar Lundh  
(co-worker of Brattemo)

*I venture after the lapse of so many years to lift the veil of discretion and reveal the fact that Katharina was not the niece but the daughter of the landlady. The girl fell ill, therefore, as a result of sexual attempts on the part of her own father.*

Sigmund Freud (GW-I:195/SE-II:134)

§114. I shall use the word "quoting" about presenting literal quotations, and the word "citing" about non-literal accounts. These definitions are of course not original.

It seems that few academicians are prone to check citations. For the most part they take for granted that citations are correct. Occasionally, they reject a citation, spontaneously and without having checked the latter—simply because of some intuitive considerations: if this highly esteemed psychologist really had said such things, he would have said something silly; consequently, he said nothing of the kind.

In actual fact, the most incredible instance of faulty citations are commonplace in the psychoanalytic literature. This is true of both original reports, textbooks, mass media articles, non-academic teaching materials, etc. But I doubt very much that this situation would still pertain, if those who excel in distorted citations, were under at least some risk of being exposed.

§115. In the present report I have already given a number of illustrations involving *inter alia* Winnicott (1975), cf. Q-3:1 and §§3-12, and Ford & Urban (1963), cf. Q-31:1. I shall now juxtapose a number of instances, most of which will also be discussed elsewhere in the first two volumes. The list could easily be extended to any desired length.

1. According to Masson (1984:5), "Freud makes it clear that usually it is a young girl who is the victim" of sexual seduction, which will involve genital or anal intercourse. And most commentators have understood Masson's text as meaning that Freud is talking about daughters or step-daughters of some 8-15 years of age, who have been forced to submit to sexual intercourse.

CHECKING WITH THE ORIGINAL SOURCE TEXT it turned out that: Freud explicitly indicates that one third of the seduced victims were males; all acts of seduction took place at the age of 2-4, while Freud entertains the strong conviction that sexual seduction after the age of 8 could not lead to any harmful consequences; the seductive acts did not involve sexual intercourse; and in a number of cases the only seducer was another young child. Cf. *inter alia* Q-285:1.

2. According to Hartmann (1959:21), psychoanalytic interpretations are invariably based on an infinity of observations. And Bonaparte (1945) derived more concrete and detailed predictions from psychoanalytic theory, than other behavioural theories may give rise to. These predictions were confirmed by a valid scientific procedure.

CHECKING WITH THE ORIGINAL SOURCE TEXT it turned out that: A patient of 42 dreamed that she "is in a small cot on the grassy slopes of a park, near a lake, looking intently at a married couple whom she knows lying quite near in their bed." From this detail alone Bonaparte inferred that the patient has during early childhood observed a couple practicing sexual intercourse in full daylight.

The patient also recounted a recollection of her wet-nurse smearing her hair with pomade and looking like a horse [because her hair looked like a horse-tail when both her hands were under the hair?] Bonaparte inferred that the patient knew that the wet-nurse had been "ridden like a horse".

The wet-nurse looked yellowish [because she was seen in candle light?] It was inferred that the "rider" was a horse-man, because horse-teeth are yellow.

Bonaparte did not apply psychoanalytic theory to identify the man in question. Her thinking was so primitive that it never occurred to her that the "rider" might have been anyone out of two known and many unknown horse-men. She arbitrarily pointed out the patient's paternal uncle.

The claim that the acts had taken place before the patient was two years old, is proved from two *a priori* principles. All children learn to speak exactly at the age of two; hence the child could have given away the couple after that age. And people invariably behave in a perfectly rational manner. They will never take the chance when the sap is rising while the child is having a nap. (Bonaparte does not explicitly state these *a priori* principles. She is not aware of her own understood assumptions.)

The valid test and confirmation were as follows. Having been thoroughly convinced of the truth of Bonaparte's construction, the patient went to her uncle who was 82 years old, told him everything, and hammered on him for months until he confessed.

(Digression: Hartmann's device was successful. None of the keen philosophers contributing to Hook, 1959, exposed this magnificent instance of mis-citation.)

3. Freud (GW-I:443f./SE-III:207) presents Wilhelm Stekel (1895) as a paper written by a child physician who has collected authentic instances of sexual seduction of children by adults.

CHECKING WITH THE ORIGINAL SOURCE TEXT it turned out that: Stekel is not at all a child physician but a psychotherapist for adults. He had made no observation of children. And he explicitly states that he will not in this paper discuss seduction by adults, but only sexual acts involving nothing but children. Then he goes on to recount a total of two cases: adults who claimed to have practiced sexual intercourse during pre-school age. Let us not take for granted that such behaviour is impossible. But since Stekel recommends that *all* children should be watched, or else there is a risk that they will do such things, we may guess that Stekel himself had convinced his patients of what they had experienced during childhood, just like Bonaparte did. Furthermore, Stekel is not in the least surprised at the patient's recollection: He and his girl-friend had practiced sexual intercourse once a week, but "One day my girl-friend announced that she had started school, and that such 'foolish actions' were not fitting for a student." (p.248) [Cf. §§422ff.]

4. Rieff (1963:11) claims that Dora sought Felix Deutsch, received psychoanalytic treatment, whence her symptoms were mitigated.

CHECKING WITH THE ORIGINAL SOURCE TEXT (= Deutsch, 1957) it turned out that: Deutsch was asked by a somatic doctor to visit Dora in her home. He did so twice, and conducted no therapy. He merely claims that an acute attack of a somatic disease had passed for the time being between his visits.

5. According to Lesche & Stjernholm Madsen (1976), Freud explicitly states in *Vorlesungen* that the goal of psychoanalytic treatment is increased self-knowledge, while symptom removal is not in the least any part of the goal.

CHECKING WITH THE ORIGINAL SOURCE TEXT it turned out that: Freud explicitly states in *Vorlesungen* that there are two and only two goals of the treatment, viz. symptom removal and guarantee against relapse. Increased self-knowledge is not in the least a goal. A microscopic part of all self-knowledge is *a means* to the achievement of the two goals, while all other kinds of self-knowledge are completely ignored.

This example will be extensively discussed in §§168–173 and 202.

6. Ford & Urban's (1963) claims have already been quoted in Q-31:1.

CHECKING WITH THE ORIGINAL SOURCE TEXTS it turned out that Ford & Urban give a severely distorted account of the nature of Freud's writings, as I have already shown in many places.

7. According to Alice Miller (1983), Freud reveals in the third seduction paper that he himself was hostile to the seduction theory, had been highly skeptical of his patients' accounts, and projected his own hostility upon the audience, suggesting that the latter was hostile.

CHECKING WITH THE ORIGINAL SOURCE TEXT (analyzed throughout the entire present volume) it turned out that: the patients never told anything about having been seduced; Freud invented the seduction events; and he was flagrantly hostile to any disbelief in them.

He accused his audience of hostility towards the seduction theory for the purpose of diverting the attention from the fact that Freud was totally incapable of producing any evidence.

8. Freud (GW-VII:208/SE-IX:174) invokes Jeremias (1906a, 1906b) in support, and presents the old Babylonian underworld as a kind of Christian hell, where people are punished because of their instinctive [MS: as well as non-instinctive] transgressions. According to the Babylonian religion, gold is the dung of the underworld. Freud uses these facts to prove that stinginess is a derivative of desire of experiencing sexual pleasure at defecation.

CHECKING WITH THE ORIGINAL SOURCE TEXTS it turned out that: the Babylonian underworld was a kind of shadow existence which was distinguished by lack of instincts and instinctual gratification. No soul would be punished; all people would indiscriminately harbour in the underworld and suffer the same fate, whether they had been good or evil in this life.

9. According to James Strachey's translation of the case-study of the rat man (SE-X:166), the idea was consciously present in the patient's mind of rats boring their way into the anus; but he was too horrified to be able to externalize the last three words. Therefore, Freud completed the unfinished sentence.

CHECKING WITH THE ORIGINAL SOURCE TEXTS it turned out that: the grammatic structure of the patient's sentence was by no means incomplete. He was talking of rats boring into the buttocks. The reference to the anus was Freud's interpretation of what was buried in the depth of the patient's unconscious mind. And there is no indication of Freud having ever told the interpretation to the rat man.

(Ch.58 will be dedicated to the analysis of this error of translation. Strictly speaking, I have not *checked* the veracity of the translation, since I read the text of *Gesammelte Werke* years before that of the *Standard Edition*. The latter text is indeterminate as to the relevant respects, but Freud's original case-notes—which were available to Strachey—will provide the solution.)

10. As if James Strachey's "improvement" of Freud's text were not enough, Kanzer (1980) adds further "improvement" of his own, and makes a dialogue out of the "incomplete" sentence.

Kanzer's contributions will be discussed in Q-135:1 and ch. 58 by CHECKING WITH THE ORIGINAL SOURCE TEXTS.

11. According to Deutsch (1957:166), Dora "had stated unequivocally: Men are all so detestable that I would rather not marry. This is my revenge."

CHECKING WITH THE ORIGINAL SOURCE TEXT (GW-V:283f./SE-VII:119f.) it turned out that: Dora said nothing of the kind. The quoted sentence is a. an interpretation, b. which was made by Freud, c. claimed to be merely tentative, and d. constructed after Dora had terminated and, hence, never told her.

12. According to Kline's (1981:401f.), Bellak & Smith (1956) described an experiment in which two psychoanalysts by means of psychoanalytic theory and on the basis of type-written dialogues predicted what would happen during the next week while two other psychoanalysts (the judges) judged after each week what had actually happened. The agreement was highly significant although far from perfect. The result is taken as support of psychoanalytic theory.

CHECKING WITH THE ORIGINAL SOURCE TEXT it turned out that: The predictors, the judges, and the supervisor of the treating analysts met regularly during the experiment, although they imagined that they were careful not to discuss any aspect which might lead to contamination between prediction and retrospective judgement. Moreover, there is no evidence that they actually used psychoanalytic theory for deriving any of their predictions or judgements. If the treating analyst tried to anger the patient, there would be little risk of predicting that the patient would actually develop so-called "negative transference". If the analyst tried to make the patient accept interpreta-

tions—cf. the second example above involving Maria Bonaparte, and Freud's behaviour toward G.de B. described in §6—he might get away with it.

This examples will be more extensively analyzed in a later volume.

13. The present example will be extensively analyzed in a later volume.

CHECKING WITH THE ORIGINAL SOURCE TEXT it turned out that: Spence & Gordon (1967) verified that two groups in a grid of 16 cells were outstanding, while all other differences were neglectible. *Consolation eaters* who had been *frustrated* and tachistoscopically presented with the stimulus "*milk*" made more *true recalls* of words such as "*suck*", "*swallow*", from a word list to be learned. *Non-consolation eaters* who had been *frustrated* and exposed to a *blank stimulus* made more *pseudo-recalls* (i.e. of words not actually found on the list), of words related to *oral pleasure*. Spence & Gordon admit in passing that these results may have a trivial explanation, but immediately go on to suggest that they provide some genuine support for the psychoanalytic hypothesis that the improved recall of the former group derived from a. the activation of an b. unconscious c. fantasy related to the d. infantile feeding situation. They completely ignore the second result about the non-consolation eaters.

*Scrutinizing a number of accounts of this experiment* it turned out that: the second result, embarrassing to any attempt of proving anything from the first result, is not even mentioned in the long summary of 497 words in Spence & Gordon's own paper. Nor is it so in the accounts given in Dixon (1971:163ff.) (1981:177ff.) and Sjöbäck (1985: 59ff.)

14. The following example will likewise be extensively discussed in a later volume. According to Roudinesco (1982) Freud met a particular strong opposition because 1. French psychiatry was in a backward stage; 2. hereditary theories were popular in France, but were rejected by Freud; 3. Janet vehemently fought psychoanalysis as an altogether false theory, but also claimed that he was the father of psychoanalysis; 4. French nationalism prevented the acknowledgement that a foreigner could make a great contribution to science; etc.

CHECKING WITH A NUMBER OF SOURCE TEXTS AND TRUSTWORTHY HISTORICAL SURVEYS (such as Freud's *Gesammelte Werke*, Ellenberger, 1970, and Janet, 1925) it turned out that: France was the indisputable leader of European psychiatry, and its fads were, for better or worse, imitated all over this continent, not least by Freud. Hereditary theories were much more prominent in Germany and Austria, even long before the Nazi movement, and they were by no means absent from Freud's own writings (cf. Q-15:1). Janet never claimed to be the father of psychoanalysis, nor did he reject this theory in toto. In fact, *Janet manifested the considerable courage of defending Freud even during the first world war.*

Hypnotherapy was the great French fad, imitated by Freud. Since 1896 it fell into disrepute in France and, consequently, all over Europe. Many therapists turned to radically different approaches, while many others merely made petty terminological or other modifications. Both groups justified the change by the claim that they had realized that some other approach is superior. Freud abandoned hypnotherapy exactly at the time when this technique was no longer profitable for *anyone's* career. He chose the second alternative and feigned to be a modified hypnotherapist; that is, he claimed to have invented an altogether new and completely different technique—which consisted of exactly the same constituents as hypnotherapy: symptoms were removed by digging out repressed events. Unsurprisingly, Freud was in France mistaken for a real modified hypnotherapist, and was therefore associated with the abandoned French trend.

15. According to Masson (1984:148), Ferenczi tenaciously insisted on the truth of what his patients told him, viz. that they had been sexually seduced during childhood.



CHECKING WITH THE ORIGINAL SOURCE TEXT (which is quoted in Masson's own book, although few readers have perceived even the most conspicuous features of these quotations), it turned out that: Ferenczi's patients did not claim to have been seduced. It was Ferenczi who asserted that they had been so, by means of the same kind of inferences as Freud and Bonaparte applied in §6 and ex.2 above.

16. Freud (GW-XIV:48/SE- XX:23) characterizes Adolf von Strümpell's review of *Studien über Hysterie* as "a harsh rejection", and in his letter to Fliess of 6.2.1896 he talks of "a vicious review".

CHECKING WITH (a part of) THE ORIGINAL SOURCE TEXT the following turned out. Masson (in Freud, 1985:171) has quoted the last third of Strümpell's review, and insinuates that this excerpt shows how correct Freud was. But within a frame of appreciation, Strümpell advances three objections to the psychoanalytic method. 1. In the hands of less responsible therapists than Freud and Breuer, the method may have unfortunate side-effects. 2. Accounts presented by hysterics and hypnotized patients may be fabrications. 3. Equally positive therapeutic effects can be achieved by other, less dangerous methods.

(Many readers may spontaneously recall the fact that psychoanalysts have for decades fought the application of psychoanalysis by therapists who are not members of IPA, on the ground that this method is dangerous. Freud himself later claimed that the accounts by his hysterical and hypnotized patients were fabricated. And in 1932 he stated that the therapeutic effect of psychoanalysis is inferior to that of a visit to Lourdes, GW-XV: 164. As we have seen in ex.5 above, Carl Lesche, a prominent Swedish practicing psychoanalyst and theorist, denies that psychoanalysts have ever claimed any therapeutic effect for their approach.—If Freud's later positions are thought to be correct, the "vicious" nature of Strümpell's evaluation would seem to consist in his having seen the truth long before Freud.) [Q-115:1]

§116. The primary aim of this juxtaposition is pedagogical: to warn the reader never to take at face value an account in a psychoanalytic paper, of another (usually also psychoanalytic) paper. A writer may be perfectly aware of the fact that Freud's proofs of his interpretations are invariably trivial, and could easily be presented in a few lines. But for persuasive purposes he may claim that these proofs are complex puzzles which because of space considerations must be left out.

§117. But note this. A printed text has a permanent existence and form. If a writer is not sure whether he has understood the text in the correct sense, he may easily re-read the paper. And if he should give an erroneous account of the paper, the possibility is in principle open to his readers of comparing the account with the original text.

By contrast, a patient's verbal statements and expressive reactions are transient, ephemeric. The observing clinician has little possibility of discovering or correcting a mistake. Readers of his account have hardly any possibility of discovering or exposing distortions. (A textual analyst may sometimes achieve that much, but very few readers are textual analysts.)

Two conclusions are suggested by this fact. First, avoiding to distort printed texts is so much easier than avoiding to distort patient productions, that anyone unable or unwilling to abstain from the former variety, must be expected to indulge in the second variety to at least as great an extent. Second, a writer who is



not sufficiently motivated to abstain from distortion in situations where there is some risk of being exposed, must be expected to abstain much less in situations in which the risk seems negligible.

In other words, we should be highly skeptical about the observation claims made by all the writers of Q-115:1.

## Freud's Knowledge of Human Nature, and the Peculiarity of Psychoanalytic Predictions

*Here, Scharnberg's knowledge of human nature is set up against that of Freud's, and the examples provided by Scharnberg do not make me take it as self-evident that Scharnberg's knowledge is superior to Freud's.*

Lars Gunnar Lundh

*[Scharnberg asserts that] All psychoanalytic predictions are false; all psychoanalytic writers reveal an unusually deficient knowledge of human nature. These formulations are hardly an indication of a sophisticated scientific feeling as to what kinds of statements could possibly be verified.*

Lars Gunnar Lundh

§118. Since the very birth of psychoanalysis, its proponents have intensively and extensively propagated the idea that all psychoanalysts are in the possession of an unusually high degree of empathy and knowledge of human nature. This idea is iterated in research reports, academic textbooks, teaching materials for children, mass media, etc. Evidently, any claim may be perceived as an established truth, if it is repeated sufficiently often.

The most remarkable circumstance is however, that hundreds of thousands of people who have iterated the idea, have read hundreds or thousands of pages of psychoanalytic literature. And in the entire literature they have never encountered one single instance of a minimum of empathy, while they have seen a veritable flood of illustrations of absence of knowledge of human nature.

§119. It may be instructive to juxtapose a set of typical statements found in psychoanalytic writings. It would be a matter of routine to extend the list to any desired length.

1. When a 14-year-old girl is kissed by force until she manages to make herself free and escape, by a man who is some 20–25 years older than her, and she does not enjoy the kiss, then this reaction alone proves that she is neurotic (GW–V: 187/SE–VII:28)

2. When a man who is some 20–25 years older, tries to seduce a 15-year-old girl, and she tells her parents what happened, this fact alone proves that she is possessed by a pathological craving for revenge. NO NORMAL GIRL WOULD TELL HER PARENTS ABOUT A SEDUCTION ATTEMPT. (GW–V:257/SE–VII:95)

3. When the girl is later forced by her father to submit to psychoanalytic treatment, with the aim of teaching her that no one has tried to seduce her (although it may LATER turn out that the psychoanalyst did not work on the basis of the father's aim); and when her 18-year-birthday takes place three weeks after the start of the treatment; and when she is given expensive birthday presents; then it is completely incomprehensible if she did not feel happy on her birthday. (GW-V:180, 219f./SE-VII:22, 59)

4. After the seduction attempt, the man in question denied everything to the girl's father, and blamed a teenager's highly-strung fantasy. This behaviour is completely compatible with the idea of the man being seriously in love with the girl, and entertaining the plan of divorcing his present wife and marry this 15-year-old girl, presumably after an engagement Period of some two years. (GW-V:183f., 269ff./SE-VII:25f., 106ff.)

5. It is absolutely impossible to feel ashamed of something one believes one has done, unless one has actually done it. (GW-I:440/SE-III:204)

[My example:] Suppose Professor Alphason was drunk at a party, and on the next day does not recall anything of what he did. A lover of practical jokes tells him that he danced naked in the street in front of a shocked crowd, which fortunately did not recognize him; he narrowly escaped when the police arrived. The joker presents hard evidence. He has had a single copy printed of the local newspaper, in which one single news item has been substituted with another written by the joker.

According to Freud's principle: if Professor Alphason feels ashamed in this situation, this feeling of his proves that he really danced naked.

6. Psychogenic vomiting could solely emerge as a result of an event involving features that are nauseous to normal people, e.g. the sight of a half-rotten dead animal. It is impossible that vomiting could derive from a railroad accident. (GW-I:431/SE-III:196)

[My example:] Nor is vomiting ever observed among prisoners who are about to be hanged.

7. When a young girl develops anxiety attacks after the experience of a young boy having stroken her hand affectionately, then it is impossible that puritan upbringing hostile to sexuality could be responsible for her symptom. Such anxiety attacks could only be the ultimate result of a previous experience of a sexual nature, such as infantile seduction. (GW-I:455/SE-III:217f.)

8. A male patient recounted truthfully that he, at the age of 4-5, was permitted to touch the sex organ of his *gouvernante*. He himself took the initiative, it was not a case of infantile seduction. The name of the *gouvernante* was Miss Robert. "Robert" is usually a boy's Christian name, and a *gouvernante* is "usually" called by her Christian name. At the age of 29 the patient did not recall Miss Robert's Christian name, and was not surprised at his failing memory.—These few facts prove that the patient was homosexual. (Freud, 1974:34-38)

9. When a young child does not defecate on the chamber-pot at the hours it

may suit his or her parents, this is one of the most trustworthy indicators that he or she will become a neurotic or at least an eccentric. (GW-V:87/SE-VII:186)

10. No one could be afraid of venereal disease (not even at a time when syphilis was incurable), except as the result of displacement of the fear of being castrated. (Fenichel, 1945:209)

11. All homosexuals "are subservient when confronted with a stronger person, merciless when in power, unscrupulous about trampling on a weaker person." (Bergler, 1971:26)

[Q-119:1]

§120. Psychoanalysis runs counter to the empirical world in each and all respects. It may be as instructive to juxtapose a set of typical predictions. And once more it would be a matter of routine to extend the list to any desired length.

1. Freud (1985:220f.) predicted that G.de B. had been the victim of fellatio during infancy. Cf. §6.

2. Bonaparte (1945) predicted that her patient had during infancy witnessed sexual intercourse being practiced in full daylight Cf. Q-115:1, ex.2.

3. Bonaparte's (1945) patient wrote a short-story when she was 7, named The Mouth Pencil. From this fact alone Bonaparte predicted, not merely that the patient had watched fellatio being practiced but that she had watched her wet-nurse and uncle doing so, likewise before she was two, and likewise in full daylight.

Note, until the 1970s Heinz Hartmann was recognized as one of the 5 greatest theorists during the entire history of the psychoanalytic movement. From the entire literature of more than 5 decades, the very best proof he could find, of a true prediction that had been externally verified, is the one supplied by Bonaparte (1945).

4. [Here, ex.9 of Q-119:1 may aptly be inserted.]

5. From the moment an individual enters psychoanalytic treatment [and, presumably, remains in treatment], he or she could never possibly develop any new symptom. (GW-V:279/SE-VII:116)

6. Individuals who are cured by psychoanalysis are provided with a guarantee for life against relapsing or developing any new psychic symptom. (GW-XI:469/SE-XVI:451)

7. Sexual seduction of a child after the age of 8, will never produce any psychopathological effect (GW-I:449/SE-III:212)

8. Individuals who as adults are distinguished by a high degree of stinginess, obstinacy and orderliness, have, during childhood, defecated in their pants at an age when most children have ceased to do so. (GW-VII:204/SE-IX:169f.)

9. When children relapse in bed-wetting after a dry period of some years, we may infer with a high degree of certainty, a. that the children masturbate, and b. that they are consciously aware of the causal connection between their masturbation and their bed-wetting. (GW-V:236f./SE-VII:74)

10. When young girls suffer from white vaginal discharge, we may infer with a high probability that they masturbate. (GW-V:238/SE-VII:76)

11. When an individual suffers from stomach-gripes, we may infer with a significantly increased probability that he or she masturbates. (GW-V:241/SE-VII:78)

12. When Dora broke off treatment, Freud predicted that she would never recover nor improve (as to her symptoms). Later, he learned that two of her symptoms had disappeared very soon after termination. (GW-V:272f., 284ff./SE-VII:109f., 120f.)

13. At the age of 18 Dora suffered from cough attacks of a duration of 3–6 weeks, and had done so since she was 12; from the loss of voice, caused by the cough attacks; from migraine, likewise since 12; from a limping foot, deriving from a fall down the stairs during childhood; from depression and animosity. But she would immediately recover from all these symptoms, if her father left his mistress. (GW-V: 202/ SE-VII:42)

14. In 1895 Wilhelm Fliess performed a surgical operation upon Emma Eckstein. Because of his irresponsibility and incompetence, she nearly died from severe bleedings, a symptom that remained for some time. Life-long suffering resulted. Because of his permanent personality, Freud was always prepared to distort the facts to make them confirm with his emotional wishes. Since Fliess was his friend, he fabricated that the bleedings were purely psychogenic. They would have emerged anyway, even if no surgical operation had been performed. (Masson, 1984)

Note, at the time of the operation, Emma Eckstein had been in psychoanalytic treatment for three years—and nonetheless developed a new and “psychogenic” symptom. Statement 5 above was written in 1901 and published in 1905.

15. In ex.12 in Q-110:1 we can see what kind of methodological acrobatics psychoanalysts need in order to produce confirmation of their predictions.

[Q-120:1]

§121. No space need be wasted upon strategic pseudo-arguments such as the following specimens. The cited instances are not representative of what is found in *Gesammelte Werke*. (Those who apply this argument wisely abstain from trying to find any instance of a different nature.) Or: Freud’s secret observations were of an altogether different nature. (This is Farrell’s argument of writer’s cramp. But ex.8 in Q-119:1 is taken from Freud’s secret case-notes.) Or: the cited instances of simplifications made *for pedagogical purpose* (Sherwood’s idea).

I do admit of a class of true predictions. For example, a psychoanalyst may heap humiliating interpretations upon a patient, and may predict that the latter will soon come to feel anger. Evidently, psychoanalytic theory was not used to arrive at this kind of predictions, nor is it needed to explain such patient reactions, nor is it supported by them.

But I see no valid objection to include into the definition of “prediction”: the assertion or guess of our future knowledge of an event which in itself may be past, present or future. Psychoanalysts may legitimately talk of predicting early

childhood events of an adult patient, just as an archeologist may predict the place of Troy etc.

§122. Almost any class of instances in psychoanalytic writings may be juxtaposed. But whence comes the idea of the superior empathy and ability to predict? In part from the simple iteration that these virtues are prominent among psychoanalysts. Sometimes *examples* are however presented. A few brief remarks on the nature of such examples, may be appropriate.

Many predictions involve nothing but non-observable entities and, hence, cannot be falsified. Other predictions are concerned with one non-observable and one observable phenomenon. If the former is predicted, falsification cannot occur. Furthermore, there are many instances in which psychoanalysts are perfectly aware of their predictions having being unambiguously falsified—but they have concealed the authentic facts. And there are instances of two different events from different periods having simply been observed, without any hint of any causal relationship between them, and without any of them having been predicted before it was observed. But the psychoanalyst may feign that one of these events was predicted from the other, and only afterwards was observed.

§123. There are many more devices, and I shall neither list all of them (until in a later volume) nor present much documentary evidence. But I cannot neglect Felix Deutsch (1957:163) summary statement: "It is striking that Dora's fate took the course Freud had predicted." To maintain this conclusion, Deutsch has to conceal each and all predictions Freud actually made, and to fabricate that the historical events which he knew had taken place decades afterwards, had been predicted by Freud.

I will say no more on all these things. It was important that the reader should see lists of psychoanalytic predictions and deductions revealing absence of empathy. When flaws are so conspicuous, the success of psychoanalysis must derive as much from the deficiencies of the academic community, as from the psychoanalysts themselves.



## A Technique For Distinguishing Deliberate Untruths From Mistakes in Good Faith: A Criterion Based Upon Probability Calculation

*It defies all logic and experience to believe that a story this detailed and complicated was the result of an honest mistake or misunderstanding.*

Jody Powell

§124. In a later volume I shall describe and justify at least 15 different techniques for disclosing whether a false statement was intentionally asserted. Unsurprisingly, more than one technique may frequently be applied to the same instance, and all of them will then support the same conclusion. In the present chapter I shall describe a single one of the criteria. To begin with, I shall define the concept of *degree of empirical commitment*. The concept is simple, and easy to grasp and apply. An *observation statement* involves a higher degree of commitment than an *inference*. And a *tentative suggestion* involves a lower degree than a *definite claim*.

Combining both dimensions, it may be asked whether a *tentative observation* would involve a higher or lower degree than a *definite inferential claim*. Interesting as the question may be from a philosophical point of view, I have not yet found myself in a research situation requiring an answer. Clearly, the answer should not be based upon logical considerations, but upon insight into the specific empirical results emerging from the conceptual choice.

§125. Freud's statements may easily be classified according to the tentative/definite and the observation/inference dimensions. For instance, *the spying event* is in Freud's original paper, as well as in my juxtaposition of Q-17:1, given in three incompatible versions. The version presented first in Freud's paper (GW-V:242 line 20–25/SE VII:79 line 33 to p. 80 line 1) is a *tentative inference*. The second version (GW-V:243 line 1–4/SE-VII:80 line 7–11) is a *definite inference*. And the third version (GW-V:243 line 16–17/SE-VII:80 line 27–29) is a *definite observation*. In other words, the three versions are presented in *the ascending order* (a second key concept) of progressively increasing empirical commitment.

Other criteria might warrant a definite conclusion on the basis of these facts alone. But the criterion of probability calculation will not justify any conclusion, unless more than one instance is compared.

§126. Numerous instances of alternative and incompatible versions of the same event or situation may be found throughout Freud's writings. As we have seen in ch. 3, carelessness can give rise to only two kinds of errors, viz. random errors and systematic errors associated with a decreased level of attention. Both these types are legio throughout Freud's writings.

A set of three versions may be presented in 6 different orders. If carelessness was the causal factor, we would expect several of these orders to be actually observed throughout Freud's writings, without any of them being appreciably more frequent than the others. But the following generalization is universally true. *Whenever two incompatible versions are presented in close proximity (i.e., on more or less adjacent pages), the versions are invariably presented in the ascending order.*

What is the chance expectation (probability) that the ascending order would invariably occur? I shall supply figures under the assumptions that *Gesammelte Werke* contains an equal number of sets of two and three versions, respectively.  $1 + 1$  sets will yield the probability 0,08;  $2 + 2$  will yield  $6,9 \cdot 10^{-3}$ ;  $5 + 5 \rightarrow 4,0 \cdot 10^{-6}$ ;  $10 + 10 \rightarrow 1,6 \cdot 10^{-11}$ ;  $25 + 25 \rightarrow 1,0 \cdot 10^{-27}$ ;  $50 + 50 \rightarrow 1,1 \cdot 10^{-54}$ .

Already the first of these figures is not altogether far from the customary level of significance, while all the others are enormous much better than the figures usually accepted as providing perfectly satisfactory proof. And the smallest figures are the most realistic ones. Throughout Freud's writings there are hundreds of sets of alternate versions. In other words, when applied to such instances as the spying event, the refutation of the carelessness hypothesis is disarmingly easy.

§127. We can be sure that the following *one-step* argument will be used for the purpose of saving Freud's honesty and undermining my criterion. To save space, I shall immediately spell out the objection completely—a thing the users of the argument would never dare to do.

1. Freud did not tell the truth when he claimed that he started to write the paper after the termination of the therapy and that he did not take case-notes during the treatment. In actual fact he wrote the case-study in a piece-meal fashion alongside the treatment. 2. On, say, 20 October, Freud had not yet any very deep grasp of Dora's mind. Hence the spying event was at that date no more than a tentative inference. Consequently, Freud should not be blamed because around that time he wrote so in the case-study. 3. On 20 November he had penetrated deeper into Dora's unconscious, and knew for certain that the spying event had really happened. 4. And around this time he looked up in the unfinished paper, found the page on which he had, one month previously, written the first version of the spying event. He mechanically added the new version, without changing a word in the immediately preceding text. 5. On 20 December he had succeeded in lifting the repression, so that Dora directly recalled the spying event. 6. Around this date he looked up for the second time, and found the right page. He mechanically added the third version. 7. It never occurred to Freud that he had thereby produced a magnificent conglomeration of contradictions. Or else, he never re-read the paper nor proof-read the proof-sheets from the printer. 8. Freud applied exactly the same piece-meal technique as regards many other sets of observations about Dora.

And this feature of Freud's text is perfectly compatible with the view that his writings are distinguished by "careful reporting of observations" (Ford & Urban, 1963:148), a "crys-

talline clarity" (Miller, 1973:500) and an unequalled "consistency and rigor" (Eissler, 1965:57).

§128. On *a priori* ground it might seem as if a universal statement (all As are Bs) involves a stronger empirical commitment than a particular statement (some As are Bs).

But the important thing is not the abstract logical relations, but the concrete psychological functions of the human mind. Both "dimensions" will coincide as regards the *modal* aspect (tentative—definite) and the *substantial* one (observation—inference); but not as regards the *quantifiers* (all—some).

It can be seen at a glance at Freud's writings, or at the first two juxtapositions (Q-14:1 and Q-15:1), that Freud's statements are by no means invariably presented in the ascending order as to quantifiers. And this fact is decisive, whatever *a priori* view we may adhere to.

But in actual fact, nothing else should be expected.

§129. In psychological respect, much less commitment is involved by the claim that *all* Freud's writings are distinguished by a crystalline clarity etc., than by the claim that exactly pp. 242–243 in *Gesammelte Werke*, (= SE–VII:79f.) are so.

Likewise, it is a rather *inexpensive* assertion that deficient sexual orgasm was found for *all* cases of neurasthenia. A much stronger empirical commitment is actually involved, when Freud claims to have given due attention to *two different variables*, and to have thereby discovered that whenever the hereditary factor is *absent*, the sexual factor is *invariably present* in neurasthenics.

§130. A quite different circumstance must also be emphasized. Many empirical generalizations stated throughout Freud's writings, have nothing whatever to do with any view he ever entertained. Nor did he want others to entertain these views. They are persuasive devices, aimed at reducing the discrepancy between his own theory and other positions.

For instance, in the case-study of Dora Freud strongly asserts that psychic factors alone can *never* produce a hysterical symptom, while somatic factors *alone* can do so; and that psychic factors are usually absent at the first occurrence of a symptom. (GW–V:200ff., 206, 212ff./SE–VII:40ff., 47, 52ff.) It is seen at a glance that each and all Freud's interpretations in the very same paper are incompatible with this theory.

It is no far-fetched hypothesis that we are confronted with an instance of the same phenomenon when Freud, in Q-14:1, claims that any hysteric has symptoms which do *not* derive from childhood. By this admission, Freud implies that psychiatrists entertaining non-sexual theories, non-repression theories, and non-infantile theories of hysteria, are not altogether mistaken. And he may hope to produce less annoyance among some of his colleagues.

We shall later encounter certain indications that the heredity theory of neurasthenia was never intended to be taken seriously.

§131. We are in a most fortunate situation, because Freud applied a specific

*persuasive technique* to the spying event, viz. *the gradual introduction of his fabrication*. If he had supplied nothing but the third and final version, many of his original readers might have been skeptical. But—a textual analyst would have been incapacitated: the latter would have had little prospect of proving that Dora never recounted any recollection of the spying event.

## Does Recent Development of Psychoanalysis Invalidate My Critical Analyses?

*Hier dacht' ich lauter Unbekannte  
Und finde leider Nahverwandte;  
Es ist ein altes Buch zu blättern:  
Vom Harz bis Hellas immer Vettern!*  
Johann Wolfgang von Goethe

*The change of clothes;  
Changed, yes,  
But the same lice of my journeying.*

Issa

§132. When errors are pointed out in Freud's writings, the objection is repeatedly raised that such criticism is besides the mark, because Freudianism has now been substituted with ego psychology or with object relationism etc. This objection may merit an analysis. But one fact should not be overlooked: those who reject the criticism with this argument do *not* admit that the errors in question are indeed found in the writings.

§133. It may be worthwhile to take a look at *the nature* of the development of psychoanalysis. One of the most prominent features of psychoanalytic writings is that, as time goes by, the amount of empirical content has progressively been reduced, and has at present reached a microscopic level. In §§33–40 I noted that both the policy of the closed door and the phenomenon of the barrister talking in front of an empty witness box, becomes more and more predominant.

§134. Strangely, a parallel development of the opposite nature is equally prominent. When *Freud's own texts* are cited, more and more empirical circumstances are read into them. For instance, Freud himself (GW-I:184–195/SE-II:125–134) merely states that Katharina's father was drunk at certain occasions. Glenn (1930b:37) "improves" Freud's text and makes an alcoholic out of the father. In ex.11 in Q-115:1 we saw Freud constructing a *tentative interpretation* of a fragment of Dora's railway station dream. Felix Deutsch (1957) made a *definite observation* out of it.

As I shall show in a later volume, *the technique of constructing observations out of interpretations, is one of the recurrent features of Freud's writings*. We have already witnessed the magnificent example of the spying event. It is an in-

interesting fact that Freud's followers have learned the technique so well, that they apply the latter also to Freud's published works.

§135. Ex.9–10 in Q-115:1 will be extensively analyzed in vol.II. Hence I am under no obligation of giving anything more than a short-hand account here. In the original case-notes Freud wrote the word "dürfte", but deleted the dots (a recurrent but no universal feature of his handwriting). In the case-study in GW the word "durfte" is found, and dots cannot legitimately be deleted in print. The probability is equal that Freud misunderstood his own handwritten word; or that he saw a way of fabricating an observation out of an interpretation. I shall now list four versions of the same event.

*First version: Freud's case-notes.*

The rat man made no incomplete sentences. He talked of rats boring into, a perfectly correct grammatical sentence in German. And he was talking of rats eating the buttocks of a victim.

Freud's critical statement: "In den After, dürfte ich ergänzen." [= into the anus, I might add.]

The critical statement is unambiguous, meaning that Freud made—probably only to himself—an interpretation of what was buried in the depth of the patient's unconscious.

*Second version: Freud's case-study according to GW.*

The rat man made no incomplete sentences. He talked of rats boring into. And he was talking of rats eating the buttocks of a victim.

Freud's critical statement: "In den After, durfte ich ergänzen." [into the anus, I could add.]

The critical statement is ambiguous. The formulation would normally suggest that Freud actually uttered the words aloud. But they are not altogether incompatible with the conception that he did not. However, there is still no hint of the words expressing anything else than an interpretation.

*Third version: Freud's case-study according to James Strachey's translation.*

The rat man stuttered and made many incomplete sentences. He talked of rats boring their way into the anus, but was too horrified to be able to utter the last three words.

Freud's critical statement: "Into the anus, I helped him out."

The critical statement is unambiguous, meaning that Freud uttered the three words aloud. And thereby he merely gave words to an idea that was perfectly conscious to the patient.

*Fourth version: Mark Kanzer's further "improvement".*

The rat man stuttered and made many incomplete sentences. He succeeded in drawing Freud into a dialogue.

[The remaining part of Kanzer's version does not differ from that of Strachey.]

[Q-135:1]

§136. One might say with some justice that everybody turned against Dora. The most esteemed theorist of the 1960s and 1970s, E.H.Erikson (1976:170), makes black appear to be white and reverses the facts: in 1922 just as in 1898–1901 "she turned everybody against everybody else."

§137. Throughout the present chapter I shall assert four main propositions. I would not conceive of the claim about the growing amount of empirical content read into Freud's texts, as a main proposition. But to the latter belongs the claim about *the steadily decreasing empirical content of original psychoanalytic writings*.



Second major proposition. *Most changes of psychoanalytic theory are intended for external use. They are not intended to have any consequence for what goes on in the seclusion of the consultation room. Their aim is to strengthen the psychoanalytic position in relation to outsiders, in debates as well as in other respects.*

Third major proposition. *A change intended to remedy a certain error, will not in the least achieve this aim. The error will remain intact, and further errors will be added by the "remedy". But outsiders may well be deceived into believing that the error has been taken care of, without any unfortunate side-effect having emanated from the action taken.*

Fourth proposition. *Psychoanalysts will sometimes claim that a set of statements S-1 had been conclusively verified, on account of two and only two circumstances: S-1 follows from a second set S-2, and S-2 has been firmly established by clinical observation. At a later stage S-2 will be abandoned, and will not be substituted with anything else that might act as support of S-1. Nonetheless, psychoanalysts will obstinately retain the claim that the truth of S-1 is as firmly demonstrated as it ever was.*

§138. In the present chapter I shall present less than sufficient support for my claims, and for good reason. Much of the support may be found elsewhere. For instance, an entire volume will be devoted to a study of what goes on in the consultation room. The present chapter has a concrete aim: to rebuke a number of *strategic pseudo-arguments*, so that my derivations and results may be evaluated on the basis of their own merits and short-comings.

§139. According to the unanimous testimony of psychoanalysts and their adherents, from the very beginning and until about the 1980s, psychoanalytic theory is as conclusively proved as any theory could be.

"We possess the truth." (Sigmund Freud in a letter to Ferenczi, quoted in Bailey, 1965: 89) [Q-139:1]

"Psychoanalysis does not permit itself to be ranged with other conceptions. It refuses to be put on an equal basis with them. The universal validity which psychoanalysis postulates for its theories makes impossible its limitation to any special sphere." (Anna Freud, quoted in Bailey, 1965: 90) [Q-139:2]

"So far, no other scientific psychology has been evolved that can match the consistency and rigor with which Freud pursued the investigation of man's personality as a system." (Eissler, 1965:57) [Q-139:3]

"The accumulated clinical evidence is positive and decisive." (Rapaport, 1965:140) [Q-139:4]

"Psychoanalytic theory [...] is adequate for clinical purposes." (Rapaport, 1965:148) [Q-139:51]

"We cannot overlook the multiplicity and pregnancy of supporting instances." (Westerlundh, 1976:1) [Q-139:6]

[In response to Janet's criticism that psychoanalytic interpretations are merely arbitrary and capricious:]

"The statements are quite worthless, for he simply does not know that the interpretations are the very reverse of this, being based on objective principles that have no reference to individual opinion, but only to the evidence of the facts themselves." (Jones, 1914:406) [Q-139:7]

§140. Furthermore, psychoanalysts have until recently agreed that only pathological defense mechanisms could prevent anyone from perceiving the truth of psychoanalysis. I do not document this statement, since most of my readers will already have encountered the latter repeatedly, and since it will be extensively discussed in several later volumes.

Exactly the same kind of arguments is prominent among highly-strung pseudo-scientists. We are certainly entitled to ask by means of what kinds of warrants psychoanalysts would justify that *their* testimonies are of a different nature. And this is the first question upon which we shall focus.

§141. Some of the just quoted writers have elsewhere made the very opposite testimonies.

"No expository and comprehensive text [on psychoanalysis] has yet been written whose correctness is accepted even by the majority of analysts." (Eissler, 1965:148) [Q-141:1]

"As things stand, there is no canon whereby valid interpretations can be distinguished from speculation, though *ex post facto* the EXPERIENCED clinician can distinguish them RATHER well." (Rapaport, 1965:141, capitals added) [Q-141:2]

"Too often the therapist came to be always right and the patient always wrong. Indeed, psychoanalytic ego psychology, too, may be considered to be a reaction to this danger." (Rapaport, 1965:143) [Q- 141:3]

§142. It is gratuitous to assert any abstract and general postulation as to what the "experienced" clinician can do. Let us try to *spell out this one-step argument*. Rapaport, Hartmann and Erikson were during the 1960s and 1970s generally recognized as the three greatest theorists since Freud and Fenichel. Either they were experienced clinicians or they were not. But in the former case, Rapaport has committed himself to the claim that Bonaparte's (1945) deductions (presented as ex. 2 in Q-115:1 and enthusiastically accepted by Hartmann, 1959), constitute valid interpretations and no idle speculation. The same is true of Erikson's (1976) interpretations about Dora (cf. §136). If, on the other hand, Hartmann and Erikson are *unexperienced* clinicians excelling in idle speculation, what must we think of the thousands of psychoanalysts who elected them into their formal and informal position?

It is also a somewhat pseudo-scientific idea that a theory may be "adequate for clinical purposes", but will not enable the clinician to distinguish between true and false interpretations. And it is enigmatic how the clinical evidence could at all under these circumstances be "positive and decisive".

§143. Likewise, it is gratuitous to criticize in public the idea that the therapist is always right and the patient always wrong. In a later volume I shall document instances of psychoanalysts who have done so, but have in the seclusion of the

consultation room behaved in a very arrogant way toward patients who did not accept their infallibility.

From the very start psychoanalysts have *at the same time asserted a certain view, and denied that they have ever asserted that view*. Numerous examples of this persuasive device will be given throughout the present report. One illustration may suffice here. Psychoanalysts have always constructed the most coarse sexual interpretations. The case of G.de B. (cf. §6.) is typical, and comparable examples may be found in psychoanalytic literature still during the 1980s, cf. Scharnberg (1984, Q-150:1, Q-150:2, Q-150:3, Q-150:4). But they have also asserted that it is merely a misunderstanding by ignorant outsiders that they are very concerned with sexuality, since they use the words "sexual" in a chaste and esoteric sense. This device for confusing the issue has been noticed already by Haberman (1914).

§144. However, if psychoanalytic theory is correct, the patient *is* always wrong *in the objective sense*, whenever any discrepancy from the analyst's view arises. Ego psychologists have invented the idea that any neurotic is in the possession of "a conflict-free sphere of the ego". But even if this idea be accepted, only the analyst is capable of discerning whether any particular patient reaction derives from the conflict-free or the conflict-ridden sphere. Hence, Rapaport has merely invented empty formulations to embellish the usual practice.

I shall anticipate the result that recent psychoanalytic interpretations are just as preposterous as Freud's original ones. An illustration. Some ego analysts want to say something nice about a patient to compensate for the flood of pejorative interpretations. Glenn accepts at face value all Freud's insulting deductions about Dora. But now listen to one of the exceedingly few *nice* interpretations added by Glenn (1980b:33). Dora was a teenager. And teenagers are distinguished by *the abstract love of truth*.

If an innocent person is sentenced to life imprisonment because of a murder someone else committed he may fight desperately for having the truth accepted. But it would be an inappropriate view that he had thereby demonstrated his love of truth as such. Evidently, it was a severe insult when Dora's family claimed that she had merely imagined the seduction attempt, and had forced her to submit to psychoanalytic treatment aiming at teaching her that Mr. K. had never tried to seduce her. Evidently, it was her right to try to change her family's behaviour. But Glenn proves his own extremely deficient empathy and knowledge of human nature, when he deduces Dora's abstract love of truth from these circumstances.

If this instance is typical, the sporadic nice interpretations of ego psychologists are as arbitrary and absurd, as their own and the orthodox Freudians' pejorative interpretations.

§145. From the point of view of the main thread of the present chapter, the above comments to certain aspects of the quotations in §139 are presented in the wrong order. Let us return to the natural start. How do psychoanalysts know that their interpretations are true?

I think three main categories of methods or criteria are suggested in Freud's

writings. The first kind of evidence consists of certain trivial words uttered by the patient in response to an interpretation. For instance, the psychoanalyst may ask: Isn't it true that so and so? And the patient may answer: "I don't know", or: "I didn't think of that." According to Freud (GW-V:230f., 217f./SE-VII: 69, 57), such verbal sequences are "very remarkable and entirely trustworthy form[s] of confirmation from the unconscious". "*No other kind of 'Yes' can be extracted from the unconscious*" (italics added).

The most systematic account of this conspicuously pseudo-scientific category of evidence is found in *Konstruktionen in der Psychoanalyse*, which will be extensively analyzed in a later volume. A glance at Freud's writings will reveal that this is the only category of evidence Freud *really* applies; he merely *feigns* to apply the other categories.

§146. According to Freud's recurrent testimony, he has for almost all his patients extracted two radically different kinds of confirmation from the unconscious. None of these two categories is pseudo-scientific, and both of them would give genuine and powerful support to concrete interpretations as well as to psychoanalytic theory. Even in the case-study of Dora, from which the just quoted testimony of the non-existence of any other kind of evidence is taken, Freud testifies that he achieved de-repression so that Dora consciously recalled the spying event. However, in *Konstruktionen* Freud suffers from "the writer's cramp" and completely forgets the "fact" that he is accustomed to observe a wealth of instance of unambiguous de-repression—despite the fact that *Konstruktionen* is written specifically for the purpose of rebuking the criticism that psychoanalysts have no genuine warrants for their interpretations.

There are many instance throughout his writings, in which Freud forgets himself and tells the truth.

§147. The second category of evidence is related to *the method of the jigsaw puzzle*. This category will be extensively discussed in a later volume. Allegedly, a wealth of observations are fitted into a whole, so that each detail is fitted together.

A glance on Freud's writings will reveal that he invariably interprets each detail in isolation, without paying any attention to other details nor to the consistency of the ensuing picture. It is simply impossible that the man who made the kind of interpretations presented throughout the preceding 146 paragraphs, could have been in good faith when he claimed to apply the very opposite method.

Evidently, Freud and his followers had no choice except to feign to apply *some* method that is not altogether preposterous—or which may be passed for not being so.

§148. The third category will eventually be used as the point of departure for a most fundamental statement. In brief the criterion of validation is as follows.

*True interpretations can be distinguished from false interpretations by the fact that the former and only the former will produce symptom removal that is permanent. False interpretations may not produce any symptom removal at all, or they may lead to transitory symptom relief to be followed soon by relapse.*

While there may some doubt as to the exact logical power of this argument: if

the empirical observations implied were in agreement with reality, the latter would constitute a strong reason to believe in the truth of psychoanalytic theory.

Unfortunately, Freud had deliberately fabricated the facts, to facilitate his career. As I shall show elsewhere: in April 1896 he claimed in public that he had cured 18 patients of all their symptoms, by producing de-repression of all the infantile causal events. In a private letter he wrote about 1 1/2 year later, he stated that he had never in his life observed any instance of de-repression, and that he hoped he would within the near future be able to cure the first patient in his life.

Until around 1960 nearly all psychoanalysts would agreed that the third criterion adequately sums up the psychoanalytic position, and that it is clearly asserted by Freud in *Vorlesungen*.

At present, psychoanalysts are no longer capable of preventing objective studies of psychotherapeutic effects. For that reason, many psychoanalysts prefer to make a volte-face and deny that psychoanalysts have ever maintained that their variety of treatment was ever intended to produce symptom removal. Cf. for instance Lesche & Stjernholm Madsen's (1976) attempt to make black appear to be white, ex.5 in Q-115:1. Later in this chapter we shall encounter another cynical attempt by these writers of confusing the issue.

But I refuse to include at this place an extensive refutation of what is no more than a strategic pseudo-argument. I shall simply refer to Grünbaum (1984, part I, ch.2), in which all escapes from the fact that Freud stated the third criterion, are closed.

§149. The distribution of the three categories of evidence is odd. The method of the jigsaw puzzle is stated in the third seduction paper of 1896 (GW-I:441f./SE-III:205). But as far as I can see, this page is unique in Freud's entire production: he *never* advanced this justification again. And as for the explicit assertion that psychoanalysts derive and justify their interpretations by means of this method, I have found a total of one reference, viz. Fenichel (1945:32), among all Freud's followers. Weak hints in passing remarks may be found here and there. But it seems that no more than 2 1/2 instances can be found in the entire literature. The half instance consists of Sherwood (1969), who dogmatically asserts that Freud weaves all details of a case into a coherent whole. Sherwood tries to induce the reader to imagine that he has indeed shown that Freud does so. Judging from reviews by some philosophers, Sherwood's trick has been successful.

§150. It seems to me that psychoanalysts have since the 1960s almost completely avoided the problem of justification. Some readers may strongly protest. But the papers from the last three decades merely discuss abstract philosophical problems around verification—problems of little significance for empirical research.

We can be sure that no psychoanalysts would today dare to defend the view that interpretations are justified by means of subsequent trivial patient responses.

Hence, we may simplify matters and discuss the development as if any one of the three categories were the only one. I shall choose the criterion of symptom removal.

§151. At the first stage psychoanalysts claimed that they knew that their interpretations are true, on account of the fact that only true interpretations will pro-



duce permanent symptom removal. False interpretations will not even produce any impressive amount of short-lived symptom relief.

At the second stage psychoanalysts abandoned the claim that psychoanalytic interpretations will produce symptom removal at all. In this situation any genuine scientist would also have abandoned the claim that psychoanalytic interpretations are known to be true, since the abandoned warrant had not been substituted with a new one.

But this is exactly what did not happen. On the contrary, the absolute truth of the interpretations was postulated as ardently as ever. Psychoanalysts took for granted that the aspect of symptom removal had no consequences for the question of the validity of the truth claim.

This is a typical pseudo-scientific attitude. And my fourth proposition (cf. §137) is well illustrated by this example.

§152. A second example will be extensively discussed in a later volume. Here I shall merely give a short-hand version. Freud explains the well-known symptom of hysterical grimaces etc. by means of the theory that the hysteric imitates grimaces made by the person who seduced him or her during infancy:

“The explanation of the phase of ‘clownism’ in Charcot’s schema of [Masson’s addition: hysterical] attacks lies in the perversion of the seducers who, by virtue of the compulsion to repeat what they did in their youth, obviously seek their satisfaction by performing the wildest capers, somersaults, and grimaces.” (Freud, 1985:218, letter to Fliess of 17.12.1896) [Q-152:1]

*This idea is the origin of the concept of identification with the aggressor.*

It is easily seen that this theory is through and through based upon *the principle of similarity*, cf. §6.

When the seduction theory was abandoned, the concept of identification with the aggressor lost its warrant, since hardly any other genuine clinical observations were substituted. But in accordance with his usual habit, Freud retained the concept. And so did his followers.

§153. There are standpoints that may be retracted with little difficulty, and others that cannot be abandoned without the whole picture falling to pieces. The psychoanalytic *core-theory* is internally consistent. 1. Any neurotic symptom is caused by repressed structures. 2. When repression is lifted, the symptom will disappear. 3. Psychoanalytic treatment is eminently suited for lifting repressions. 4. Consequently, psychoanalytic treatment will usually produce symptom removal. 5. Symptom removal following psychoanalytic treatment will necessarily be permanent. Moreover, the patient will also be provided with a guarantee for life against ever having any other mental symptom.

A few corollaries may be added. 6. Non-psychoanalytic therapy, e.g. behaviour therapy, will not lift repressions. 7. Hence, non-psychoanalytic therapy will not lead to symptom recovery. 8. Non-psychoanalytic therapy may occasionally lead to *repression* of the symptom itself. But since the *need* of manifesting a symptom is unimpaired, either the same symptom will soon return, or a different and more serious symptom will be substituted.



After the publication of Eysenck (1952, 1960a) psychoanalysts started furious and protracted attempts to suppress facts they had since the 1890s always known to be true. But eventually they resigned themselves. They mechanically cut out the 4th and 5th statements of the core theory, and substituted them with the true statement, viz. that psychoanalytic treatment will not produce symptom relief—with the corollary that behaviour therapy may well produce permanent recovery with no unfortunate side-effects.

As a result, the new theory is a conglomeration of two incompatible half theories which have been mechanically juxtaposed. Substitution did not produce any reasonable degree of agreement with the empirical facts. But the internal consistency was destroyed.

§154. An analogous example is provided by Freud's paper *Konstruktionen in der Psychoanalyse*, I shall give only a short-hand presentation, since this paper from 1937 will be extensively discussed later. Recall from §148 that Freud himself has testified that he told the very opposite of the truth in his report read at the meeting of the *Association for Psychiatry and Neurology in Vienna* (= the third seduction paper): it was not true that he had cured any of the 18 patients, nor had he observed any instance of de-repression. Note also that the case-study of Dora contains a total of one instance of de-repression, viz. the spying event, and exactly this instance is unambiguously faked.

In *Konstruktionen* (GW–XVI: 53/SE–XXIII:265f.) Freud admits that “FREQUENTLY” (hence, by implication: *not invariably*) a psychoanalyst will not succeed in producing de-repression of the original causal event. Instead, he may produce a firm conviction in the patient, according to which the postulated event has really happened and is causally responsible for the emergence of the symptom. And this patient conviction will produce the same effect as lifted repression.

§155. According to the original version of the theory, a considerable amount of energy is lost by repression and symptoms. The energy is prevented from reaching its natural outlet along the natural route. The repressing obstacle may also consume some amount of energy. And so may the symptom itself. But in *Konstruktionen* we are told that it does not matter at all that all the structural obstacles to sound and smooth function remain intact, if only the patient is aware of their existence and nature.

Since Freud was very fond of hydraulic models, the reader may try to imagine a house severely malfunctioning because of stoppages in the water- and sewage-pipes—but where a firm conviction of the inhabitants as to the cause of the defects will automatically remedy these defects, even if the stoppage remains intact.

*Another aspect should not go unnoticed. How would a patient arrive at a firm conviction as to what goes on in the depth of his unconscious, and what forgotten events occurred during his infancy?* Evidently, he can neither perceive nor feel such things. Likewise, it is impossible to doubt the psychoanalysts' testimony that they do not produce convictions by presenting the patient with such logical derivations which might convince a genuine scientist. (I shall disregard the fact

that their derivations are invalid.) Flagrantly, only one alternative is left. They apply irrational techniques for producing irrational beliefs which could only by a miraculous coincidence be veracious.

Once more we have a conglomeration consisting of two mechanically juxtaposed incompatible half theories.

§156. Why did Freud invent such a theory, since he already had an internally consistent theory? His motive is not difficult to guess.

As the number of psychoanalysts grew, it must have been increasingly more difficult to maintain the original position that de-repression is invariably produced by psychoanalytic intervention. The same difficulty will turn up as regards symptom removal. Numerous enthusiastic practitioners may wonder why they *never* observe in their patients what the greybeards claim to observe invariably. Worse still: the young practitioners will have undergone a personal psychoanalysis conducted by the pioneers. Hence, each of them will have been an eye-witness of the fact that a greybeard made quite different clinical experiences with at least one patient.

The theory of *Konstruktionen* was presumably intended to remove the discrepancy between the theory and the real state of things. Logically spoken, it does nothing of the kind.

*Konstruktionen* was published in 1937, and *Vorlesungen* in 1917. *Unsurprisingly*, in *Vorlesungen* Freud claims to have made clinical observations to the effect that the theory stated in *Konstruktionen* is erroneous:

“If the doctor transfers his knowledge to the patient as a piece of information, it has no result. [...] It does not have the result of removing the symptoms, [...] The patient knows after this what he did not know before—the sense of his symptom; yet he knows it just as little as he did.” (GW–XI:291/SE–XVI:281) [Q-156:1]

§157. I will not devote many words to the theory of female psychology. But a few facts are crucially important. No psychoanalyst would during three generations have admitted that Freud’s doctrine of female psychology is less firmly rooted in clinical observations and less conclusively verified, than any other important doctrine. Pathological reality-distorting mechanisms were automatically attributed to skeptics:

“Thus we shall not be so very greatly surprised if a woman analyst, who has not been sufficiently convinced of the intensity of her own desire for a penis, also fails to assign an adequate importance to that factor in her patients.” (GW–XVII:127/SE–XXIII:197) [Q-157:1]

Recall Rapaport’s statement in Q-141:2 that experienced clinicians can distinguish rather well between valid interpretations and speculation. Who was the experienced analyst in the present case?

One aspect of Freud’s theory is not always noticed. It is not merely a preposterous and sex-centered idea that there is something particularly *enviable* about possessing a penis. This idea also belongs to a *very primitive type of personality*.

§158. A second crucially important circumstance. *The doctrine of female psy-*

*chology was by no means abandoned because psychoanalysts discovered a number of new data, nor because they learned anything new about the true implication of their previous data. The doctrine was retracted solely because the Women's Liberation Movement eventually had grown so strong, that psychoanalysts would have compromised themselves by retaining the doctrine.*

It is to be expected that some psychoanalysts will reject this account. I challenge them to present even one clinical observation which emerged after 1960 and was unknown to the entire profession until that year, and which has motivated any psychoanalyst to abandon the doctrine. When one is totally devoid of any non-trivial observations, it is an effective trick to assert that "it is *all* the observations which *together* prove this or that". But the analysis of the persuasive techniques applied by Freud and his followers belong in another volume.

The present instance is related to my second proposition, but only partially so. For obvious reason, psychoanalysts had no choice except to change both their external appearance and their behavior in the consultation room, as regards the doctrine of female psychology.

§159. Many other changes have been made solely for the purpose of improving the external reputation, while they are intended to leave everything intact in the consultation room.

Scientifically minded psychologists may ask a most embarrassing question: How do you know that your interpretations are true? And if you do not know, how dare you try to induce your patients to believe in them?

Until very recently, psychoanalysts would give an answer which some philosophers have taken to prove that they are sophisticated methodologists — viz. that they are in the possession of a wealth of secret evidence of absolutely conclusive power, and also in the possession of a number of absolutely valid secret methodological procedures, by means of which they can with absolute certainty rule out any kind of contamination as regards the data produced by the patient and the way in which he came to accept an interpretation. And if the skeptical psychologist should undergo a personal psychoanalysis, he would understand everything.

Some recent psychoanalysts have however substituted this stratagem with a more intelligent answer of the following nature: I do not know that my interpretations are true. I merely present them to the patient as tentative hypotheses. And then the patient may make up his own mind as to whether to accept or reject them.—Having won a debate by means of this device the psychoanalyst may return to the consultation room and behave as dogmatically as any of his predecessors did. Concrete illustrations of this double-talk will be supplied in due course.

§160. This is a genuine instance of the type of innovation described by my second proposition. And as usual, the new theory consists of two mechanically juxtaposed incompatible half theories.

Few psychoanalytic ideas are more fundamental, than the claim that anyone suffering from emotional trouble is likewise the victim of a *cognitive* defect.

Talking of Dora, Freud states the following generalization: "an impartial judgement (of which, indeed, the patient is incapable)". (GW-V:219/SE-VII:58f.) This principle is as much alive in the 1990s as it was in the 1890s. One might say that *no psychoanalytic proposition is more indispensable*. Psychoanalysis may accommodate itself to the view that one or the other kind of interpretations is true. But it could never renounce the position that the interpretations actually delivered are true, and that the patient's refusal to accept them is a pathological reaction that must be conquered. The mere presentation of tentative hypotheses would have no effect, since the patient would necessarily reject all true interpretations.

In *external* discourse, the last device involves a *reduction of the degree of empirical commitment*: definite inferences or observations have been substituted with tentative inferences.

§161. While it is a most prominent feature of psychoanalytic methodology that *clinical observations and empirical generalizations are fabricated ad hoc whenever they are needed*, it is another feature that *clinical observations and empirical generalizations which were previously asserted to have been abundantly and conclusively verified, may disappear in silence and ad hoc*.

In 1896 and some 10 years ahead, it was allegedly a meticulously verified fact that all hysterics will claim that they have been sexually seduced. But from the moment Freud substituted the theory of the historical authenticity of these events with the fabrication theory, Freud never encountered any single hysteric who claimed to have been seduced.

Analogously, Freud had in his youth collected an abundance of observations to the effect that masturbation is highly frequent around the age of 8. Later, he asserted the empirical generalization that any manifest sexual activity has disappeared around this age.

During at least 1896–1917 Freud had allegedly gathered an abundance of observations of the immense therapeutic power of psychoanalytic treatment. In 1932 he boasted of the fact that the recovery rate is lower than that of a visit to Lourdes. (GW-XV:164/SE-XXII:152)

It would be a matter of routine to list dozens of comparable instances.

§162. Next, I shall give a particularly illuminating illustration of the diminutive difference between psychoanalytic interpretations in 1926 and 1976. But I take the liberty of discussing also a number of subsidiary circumstances which, although important in themselves, are not very relevant in the present context.

Herman Nunberg belongs to the middle generation of the psychoanalyst trained by Freud himself. He was once considered one of the greater but not greatest theorists. And still in the 1970s his writings were occasionally used in academic courses. Above all, he is the originator of the most fundamental "administrative" feature of psychoanalysis—a feature without which this theory would probably not have survived for more than one generation. Viz., the condition that each psychoanalyst must undergo a personal psychoanalysis. Since the idea has for more than 60 years been incessantly propagated that this variety of a

personal experience will guarantee the freedom from cognitive as well as moral prejudices, some space may legitimately be devoted to the subsidiary question whether Nunberg himself was free from prejudices. I shall first present the set of observations invoked by Nunberg, together with the interpretation he considers proved by this set.

"We have the case of a man of thirty-four, who fell in love with a married woman, the mother of six children and the wife of a friend of his. With her he was impotent and at the same time became so with other women. He came to analysis with the desire to become potent with this particular woman; he did not mind about any other. He wanted to separate her from her husband, whose death he desired." (Nunberg, 1976a:186, originally published in 1926) [Q-162:1]

"The patient, a cultivated and in other respects an intelligent man, could not perceive the folly of the situation and expected from the treatment the realization of his infantile wishes. Thus his unconscious endeavour was to remain infantile, but it was the duty of the physician to free him from his infantile fixations.

As a rule impotent men expect from the treatment not average, normal potency, but [...] nearly always hyperpotency." (Nunberg, 1976a:186) [Q-162:2]

§163. Today, it is easily seen to be a primitive prejudice that a female having children with another male, should somehow be an inferior object for love and marriage. But it is a more crucial fact that Nunberg is incapable of *distinguishing* between inferences implied by his psychoanalytic theory, and conclusions deriving from his private moral prejudices.

Moreover, scarce space need be wasted on the fact that Nunberg's incest interpretation has been derived by means of *the principle of similarity* (cf. Q-6:1). *The illusion of separation* (cf. Q-186:1) is also very much alive. But if anyone loves a female with a certain property, it is an invalid conclusion that he loves her *because* of this particular property.

Although the comprehensive presentation of psychoanalytic methodology must be postponed to a later volume, I shall now present *the psychoanalytic standard operation procedure*. Nunberg can easily be seen to have applied the first three rules of this procedure.

1. Start with a preconceived interpretation.
2. Pick up a few details here and there on the criterion that they can be used or misused to support the interpretation.
3. Connect them with the interpretation by means of the principle of similarity.
4. Ignore all data which cannot be used as pseudo-support of interpretations.
5. If data which contradicts the interpretation have inadvertently been obtained, suppress them and conceal them from the reader.

[Q-163:1]

§164. The insinuation that this patient expected hyperpotency from the treatment, does not tally with the fact that he did not mind whether he would remain impotent with all females but one. There is a contemptuous flavour about Nun-



berg's formulation, and we may guess the reason. Nunberg (1976b:197) explicitly states that a *psychoanalyst* "*promises the patient at the introduction of the treatment to cure him*" (italics added). And numerous individuals have read in texts directly or indirectly emanating from psychoanalysts, that this variety of treatment is eminently suited for curing impotence.

Today it is a well-known fact that psychoanalysis will not cure impotency, although occasional instances of spontaneous recovery may occur while a patient happens to undergo any kind of treatment.

§165. During 100 years psychoanalysts have had the habit of blaming the patient for their own failures. For instance, Freud stated the following principle in 1909:

*"In my practice, I am chiefly concerned with the problem of repressed sadism in my patients; I regard it as the most frequent cause of the failure of therapy. Revenge against the doctor combined with self punishment."* (Letter 163 to C.G. Jung, quoted in Bergmann, 1976:33, italics added) [Q-165:1]

Freud applied the same principle to explain away why Dora did not recover, shifting the blame to others. How fundamental this tendency of shifting the blame to others was in Freud's personality, may be illustrated by the fact that he did not feel ashamed of stating *in print*: "I may here go back a little and explain how it was the fault of my fiancée that I was not already famous at that early age." (quoted in Bailey, 1965:14).

The principle is also found in Rollo May (1961:292f.) And Wolpe (1976:190) notes the problem of treating patients who have previously been indoctrinated by psychodynamic therapist to believe that it is their own fault that they have not recovered. I shall also quote a statement made by a patient treated by a most recent object relationist: "I know what I am doing is self-defeating, but to stop thinking and acting in that way would require that I become someone else and I can't do that. I wouldn't recognize myself when I look in the mirror." (Ogden, 1986:158). [Q-165:2]

*The principle of prestige*, one more fundamental constituent of psychoanalytic methodology, will be defined in Q-186:1. The latter is evidently responsible for the psychoanalytic attempts at explaining away their failures. To sum up the present section, there is room for the suspicion that Nunberg's arrogance against his patient derived from exactly this aim of explaining away his incapacity of keeping his own promise of curing the patient of impotency.

§166. We have now come through the subsidiary issues, and may proceed to the main aspect.

Martin S. Bergmann & Frank R. Hartman (1976:182, 186) are prominent ego analysts. They accept every aspect of Nunberg's Oedipal interpretation. They take it as a fact that the patient conceived of the beloved woman as his mother, and the analyst as his father; and that he wanted his father's permission to sleep with his mother. Bergmann & Hartman have added a total of two new ideas. First, they think that Nunberg should have postponed delivering the interpretation until the patient was ready for it.

Second, Nunberg saw it as his "duty" to induce the patient to abstain from the



beloved woman. But the ego analysts think that the patient should rather be told that the woman is not his mother and that the analyst is not his father, wherefore the analyst can neither forbid nor permit anything.

§167. One aspect of the ego analytic interpretation must not be overlooked. It is a universal feature of psychoanalytic writings to leave out crucial information. If the ego analytic interpretation had been presented in a straightforward way, we can be sure that almost all patients in the situation under consideration would have answered: I know that this woman is not my mother and that you are not my father. I have not come here to obtain your permission to sleep with her, but to regain potency.

It would be no use to deliver the part of the interpretation that is new to ego analysis, *unless* one had first induced the patient to believe in the part that is common to orthodox Freudianism.

The example of the last six paragraphs certainly suggests that stagnation rather than impressive development is the distinguishing feature of psychoanalysis.

§168. We have now arrived at the penultimate subject of the present chapter. I cannot altogether neglect *the hermeneutic re-interpretation of psychoanalysis* made by Jürgen Habermas (1968) (I have read nothing by Paul Ricoeur). But after Grünbaum's (1984) extensive analysis I need say only a few further words.

In the Western world there is a long tradition of some half millenium of misusing something belonging to the borderland of philosophy, to defend this or that strange view. As we have seen, Lady Welby invented a new semantic method. Being a proponent of negro slavery, she "discovered" that the opponents were wanting in clear thinking and made "semantic errors": they confused two quite different things: "being a slave" and "living an unpleasant life". (Statistics shows that a negro slave imported from Africa to the U.S.A. would live on the average 10 years before he had been worked to death. Hobhouse, 19~35.) I shall list numerous comparable examples in a future separate report on the criticisms advanced against the experimental method.

§169. The presentation of Habermas theory I shall give in a moment, is peculiar in certain respects. It is definitely not intended as a parody, nor is it some kind of simplification. It is as exact as it could possibly be. And the fact that it does not consist of abstract statements that are not altogether easy to understand, does not reduce its exact character. I have added a concrete embodiment of Habermas' model, and my purpose is pedagogical. A reader may miss all the crucial points, when reading a text by Habermas himself or by some of his proponents. After my embodiment, any reader will easily grasp the nature of Habermas theory, and will easily detect the weak points.

I shall apply the model to a type-writer. To simplify the discussion, I shall assume that the type-writer will always function as it should, if one particular spindle is straight, while it will not function at all if this particular spindle is bent. The reader will easily see that: if my account is veracious *with* this assumption, it will be so also *without* the latter.

On 2.2 (2 February) the type-writer is functioning in an adequate way. On 3.3. someone drops it on the floor. A service-man is eventually called. He does something to the apparatus, and on 4.4 the type-writer is again working satisfactorily.

The natural interpretation of this sequence of events could hardly be more obvious:

A. The law-like statement or, for short, *the law* "If the spindle is straight the type-writer will function as it should; and if the spindle is bent, it will not function at all"; this law is true on both 2.2, 3.3, 4.4 and henceforth.

B. On 2.2 *the empirical fact* "The spindle is straight" is true. This fact together with *the law* will lead to another *empirical fact*, viz. "The type-writer will function as it should".

C. On 3. 3 *the empirical fact* "The spindle is bent" is true. This fact together with *the law* will lead to another *empirical fact*, viz. "The type-writer will not function at all".

D. Between 3. 3 and 4.4 the service-man has straightened out the spindle. That is, he had *substituted one empirical fact with another empirical fact*. He has *not changed the law*.

E. On 4.4 *the empirical fact* "The spindle is straight" is again true. This fact together with *the law* will lead to another *empirical fact*, viz. "The type-writer will again function in an adequate way. "[Q-169:1]

§170. Let us now see how Habermas' interpretation differs from the natural interpretation.

Until the service-man arrives, there is no difference. But according to the hermeneutic re-interpretation, the service-man performs two quite different tasks. One of them is that he straightens out the spindle. The other is of a more occult nature: *he nullifies the law*.

Consequently, the following three statements are true on 4.4. The spindle is no longer bent. The law is no longer true. And the type-writer is functioning as it should.

The reader will immediately realize that Habermas has attributed a strange property to the service-man: *he has provided the type-writer with a guarantee for life against relapse into any kind of defect that would interfere with smooth functioning*.

§171. Lesche & Stjernholm Madsen (1976) have produced a genuine improvement of Habermas' theory. According to their view (if applied to *my* model), the spindle is not really bent, nor is there really anything wrong with the function of the machine. Instead, the type-writer *erroneously believes* both that the spindle is bent and that adequate function is impossible. Consequently, the task of the service-man is simply to substitute the type-writer's erroneous belief with the corresponding true belief.

Why is this theory so much better than Habermas' version? Because Lesche & Stjernholm Madsen have introduced the factor of a subjective belief. Because of

this innovation, they are *entitled* to apply their model to human beings, while denying that it is applicable to type-writers.

As a matter of actual facts, Habermas does reject the application of his theory to type-writers. But the theory is formulated in such terms that this rejection is purely arbitrary—apparently motivated by the fact that anybody would detect the error if he permitted this application, while it might be a little less transparent that the application to human beings is equally absurd. (Compare Habermas' qualification with the following statement: *Any object that is oblong and thin, is combustible; but this statement is not applicable to steel needles.*)

§172. Lesche & Stjernholm Madsen's theory has no merits other than the internal consistency. They explicitly apply the latter to the nature of psychotherapy/psychoanalysis. The patient *erroneously believes* that he is the victim of some kind of causal necessity, when he is afflicted with anxiety attacks, impotence, despair etc. But in actual fact he himself has created his own rules. And he may arbitrarily change them—thereby ceasing to suffer from anxiety attacks, impotence, despair etc.—just as easily as he might change dinner-time.

§173. It would be interesting to see the writers apply their own theory to tape-recorded dialogues from the consultation room. They do not explicitly shift the blame for non-recovery to patient. But the accusation is no less clearly implied. If an impotent male after 6 years of treatment complains of not having recovered, a psychoanalyst would, according to the hermeneutic theory, be *perfectly correct* if he answered: "But it is your own fault. You could easily have recovered 5 years ago or more, if you only had wanted to." Hence, there is little difference between Freud, Lesche, and Ogden.

Note that the theory exposed by Lesche and Stjernholm Madsen has nothing in common with the theories stated in the writings by Freud, Fenichel, Rapaport, Winnicott etc.

Nonetheless, *the theory is in much better agreement with the actual clinical practice of these psychoanalysts, than with their own writings—apart from occasional slips of tongue in these texts.*

Note that there is no room for any unconscious processes in Lesche & Stjernholm Madsen's theory—or at least not for any kind of unconscious causal factors influencing conscious processes, nor for any kind of unconscious phenomena which psychoanalytic intervention may not safely neglect.

These writers have unwarily arrived at a standpoint very similar to mine:

*From the case-study of Dora, inter alia, it, it is easily seen that THE UNCONSCIOUS HAS NO PLACE IN PSYCHOANALYTIC TREATMENT. THE LATTER PROCEEDS ENTIRELY AS IF THE UNCONSCIOUS DID NOT EXIST. Freud's sole goal is to SUBSTITUTE DORA'S CONSCIOUS BELIEFS WITH OTHER CONSCIOUS BELIEFS. And his only technique consists of hammering his interpretations in the most brutal way. [Q-173:1]*

Evidently, nothing hinges upon whether a psychoanalyst may apply more sophisticated persuasive techniques to exchange the patient's beliefs.

§174. We have now arrived at the last main subject of the present chapter.

At the first stage of psychoanalytic theory, the goals of the treatment were unambiguously two: to achieve symptom removal and guarantee against relapse. And the interpretations were presented as nothing but *a means* for attaining these goals.

Eventually, it became too well-known that the treatment has no therapeutic effect. Freud did what so many cranks and administrators of holy sources had done previously: he retained the treatment, invented new goals, and fabricated the empirical generalization that it had been abundantly verified that the treatment will indeed achieve the new goal. And the latter consisted of self-insight and self-understanding.

§175. Extremely few patients will not have *more* unrealistic beliefs and *less* self-understanding *after* a personal psychoanalysis. But as time went by, it became increasingly more difficult to conceal the fact that psychoanalytic interpretations are simply not true. Once more, the treatment was retained, and new goals were substituted.

At the third stage *psychoanalytic interpretations were simply conceived as consolation lies*. And this is what in the end came out of a treatment allegedly designed for curing neurotic symptoms, and then justified by reference to the immense respect of truth manifested by the analyst and taught the patient.

§176. At present, only a minority group adheres to the doctrine of interpretations as consolation lies. But the same thing was once true of Neo-Freudian ego psychology and of object relationism. The fundamental shortcoming of the doctrine does not differ appreciably among different representatives. A typical and rather lucid account of the position is found in Lundh (1983):

“Loch (1977), a psychoanalyst, distinguishes between two notions of truth; truth in the scientific sense, understood as correct statement; and truth as the emergent, the construction of something that makes sense that therefore permits one to rely on it and to continue living” (p.221). Psychoanalytic interpretations, according to him, ‘are not so much concerned with meaning comprehended as a fixed signification (truth in the scientific perspective), but are meant to open possibilities. They enable the patient to discover and construct new meanings, and they do this by freeing him from any fixation’ (p.245–246).” (Lundh, 1983:71f.) [Q-176:1]

“Freud’s concept of ‘transference’ refers to what seemed to him to be a tendency on the part of the patient to transfer his past emotional attachments to the psychoanalyst. This transference of patterns that have their origin in the patient’s childhood and which relate to his parents, [...]” (Lundh, 1983:73) [Q-176:2]

“The psychoanalytic situation provides ideal conditions for reliving infantile emotions. The patient comes asking for help, and this fact puts him into a dependent, child-versus-parent relation with the analyst. The analyst’s amiable, yet nonparticipative listening contributes to the patient’s feeling of having found a friendly, benevolent, omniscient parental substitute.” (Wolman, 19683:176, quoted in Lundh, 1983:73) [Q-176:3]

§177. Only the first quotation belongs to the innovative part of the doctrine. In accordance with the standard pattern, this doctrine consists of two mechanically juxtaposed half theories. The previous defects of psychoanalytic theory have

neither been remedied, nor even reduced. But new ones have been added. Incompatible old propositions are retained alongside with the recent ones. I shall first make a point which, although of considerable importance, is not highly relevant in the present context.

It is not true that "asking for help" will usually put a person "into a dependent, child-versus-parent relation". Many of us have asked for help from lawyers, somatic doctors, driving teachers, TV service-men, and so on; and few of us have perceived such people as parental substitutes. It cannot be doubted that numerous patients in psychoanalysis show strong dependency. But this attitude must *obviously* have a quite different root. Moreover, patients' *initial* attitude to their psychotherapist or psychoanalyst is known to show considerable variability. Some patients perceive the therapist as a kind of servant who must do a certain job for them. (Terruwe, 1959:121, 1960:132).

§178. No one has ever performed an empirical study and shown that psychoanalysts are amiable; that they listen in a nonparticipative way; or that they are perceived as friendly, benevolent and omniscient. And if we do encounter a case in which all these properties are found, it is by no means obvious that "amiable nonparticipative listening" is causally related to the emanation of ideas of an "omniscient parental substitute".

Freud was arrogant and insulting toward Dora, and he hardly listened at all. Rather, he behaved like a brutal police interrogator who delivered his interpretations before any evidence had emerged, and put Dora under very hard pressure to confess. Bergler (1958, 1961, 1971) was hyper-aggressive, non-listening, arrogant, and insulting toward all his patients. An entire volume will be devoted to the study of the interaction in the consultation room; but I shall also elsewhere comment up these facts. Haley (1963) presents psychoanalysis as essentially a *punishing* experience. In the present context I need not take any stand as to whether Haley is correct.

§179. Freud and all his followers until around 1960 have always maintained that their theory and interpretations are true in the scientific sense. They have attributed pathological blindness to skeptical critics. Still today they force interpretations upon their patients, in a way which would be manifestly unethical, unless they had sound reasons for taking these interpretations as objectively correct.

Moreover, if psychoanalysts during some 7 decades were in the possession of a wealth of conclusive secret evidence, how could the idea ever have occurred to them to take Loch's paper seriously?

§180. It is an instructive fact that no psychoanalyst has discovered that it is a sheer impossibility to apply Loch's doctrine to the interpretations found throughout the psychoanalytic literature. No one seems to have compared these interpretations with the doctrine. They are the very opposite of consoling. In general, they will close possibilities, produce fixations, and deprive the patient of meaning.

It will be impossible to rely on them (except at a more primitive level), and they will make it more difficult to continue living.



§181. I shall illustrate how the application of Loch's theory may look.

Mr. K. tried to seduce Dora when she was 15 years old. She rebuked him. Afterwards, he and her father agreed that the seduction attempt had only taken place in her own highly-strung imagination.

Freud presented her with the following interpretations. Dora was deeply in love with Mr.K., and had given him many proofs of her love. [Freud wisely abstains from presenting any of these proofs.] However, she did not dare to submit to her own feelings, because she feared that her sex organ would be repulsive to M.K., as a result of her [alleged] childhood masturbation. Her cough attacks [which actually emerged some three years before her love] were caused by her wish to suck Mr.K.'s penis.

§182. There is some logic in the following view. It may be very painful for Dora to believe in the above interpretations. Nonetheless, there is no way out, because her symptoms will disappear if and only if she believes in them.

It is a quite different matter to defend the following alternative conception. It is precisely these interpretations that are fictive constructions. And attempts are made to induce Dora to accept them because they will make sense of Dora's reactions to herself; because they will permit her to go on living by relying on them; because they will free her from fixations; and because they will open possibilities to her.

Besides, Dora was in no need of any new ideas for going on living; and her pre-therapeutic ideas made as much sense as anyone might wish.

§183. I shall add an even more crucial point. *If the recent conception of interpretations as consolation lies be accepted, the concept of "transference" will be self-contradictory.*

In the following account of a small sequence of events involving Dora, I have substituted a few details supplied by Freud with my retrieval of the authentic historical occurrences. The reader will have to wait for documentary support until the appropriate volume has been published. However, nothing hinges upon whether one or the other set of details be used as the point of departure.

According to my reconstruction, Dora had been forced to submit to psychoanalytic treatment for the purpose of inducing her to accept that the seduction attempt had only taken place in her own imagination. This was certainly a strong insult. And it was the fact that her father sided with Mr.K., that had produced her depression and animosity toward her family.

Things turned out in an unexpected way. Freud accepted Dora's version that the seduction attempt had really occurred.

Consequently, Dora could eventually present *a new argument*. Her father and Mr.K. had selected a doctor *they* trusted—but it was precisely this doctor who had accepted *her* version. Confronted with *this* argument, they dared no longer stick to the version they had always known to be false. And as soon as they admitted the truth, Dora's depression and animosity against her family disappeared. Moreover, she terminated the treatment primarily because she had obtained the new argument; but perhaps also because remaining in treatment would have im-



plied an admission of the "fact" that she was in need of being "cured" of her "fantasy".

§184. Freud was perfectly aware of these causal relations, but he deliberately tried to conceal them. And to do so, he deliberately distorted *the temporal relations*. He met Dora for the first time about one week *after* the seduction attempt. But on one page he claims that the meeting took place *some two days before* this event, and on another page, *some two months before*. Likewise, Dora's depression and animosity disappeared *4-5 months after termination*, but Freud says *4-5 weeks*.

How did Freud explain Dora's termination and partial recovery a while after termination? He claims that Dora "*transferred*" her love of Mr.K. to Freud. She also "*transferred*" her inability to submit to this "transferred" love. And she "*transferred*" the escape reaction: just as she had left Mr. K. abruptly when he tried to seduce her, she left Freud abruptly. Freud goes on to explain that the cessation of Dora's depression and animosity was the *direct* result of Freud's interpretations (of which we have seen or shall see many specimens in §§ 16f., 45, 115, 119, 211, 323ff., 410f.). But because of Dora's "*transference*", recovery could not have an outward expression. When she terminated, her "transference" was gradually reduced. Consequently, the *latent* symptom recovery achieved *during* the treatment, was transformed into *manifest* symptom recovery.

§185. Freud does not realize that what he here presents is an entirely new theory. If the latter is true, psychoanalytic theory is altogether false. It is neither necessary to achieve de-repression so that the patient can directly *perceive* the facts asserted by the interpretation (as postulated by the psychoanalytic core theory), nor even to produce a firm conviction in the patient (as postulated in *Konstruktionen*). The analyst may simply *deliver* the interpretation, and the patient may *reject* it as wholly inappropriate—and then the patient will lose his or her symptoms.

None of the psychoanalytic commentators on Dora have detected the *non-psychoanalytic* position asserted by Freud in the last chapter of the case-study.

A second consequence of the new theory is that it might often be profitable to terminate treatment prematurely, or to alternate between periods of treatment and non-treatment—a consequence never drawn by Freud. There is no sign in the entire case-study of any transference love, nor of any other variety of love, nor of any escape reaction in relation to Freud. And since Dora flagrantly was not in love with Mr.K., and never tried to escape from her feelings, she could not possibly have "transferred" such feelings and reactions to Freud.

§186. How did Freud arrive at the interpretations? Truth may be claimed for a part of the following explanation with certainty, and for another part with a very high degree of probability.

Unambiguously, all these interpretations are based upon *the principle of similarity*, just like the interpretations of G.de B.'s eczema, Dora's asthma, and the dream of Bonaparte's patient (Cf. §§6, 17, 115). There is indeed a marginal and irrelevant similarity between leaving a man in anger after a seduction attempt,

and dropping peacefully out of psychoanalytic treatment when one is no longer forced to obey one's father's command of remaining in treatment.

Furthermore, the entire psychoanalytic literature contains no single instance of *psychoanalytic rationalization* being attributed to a patient, for which the evidence is not enormously much weaker than the evidence supplied by the present instance. Freud's interpretations will indeed function *to himself* as consolation lies. Freud can "rely on them" and "continue living". They will "make sense" or rather, they will suggest a sense which will put Freud in a favourable light.

In the chapters on *the canon of psychoanalytic methodology* I shall present 7 fundamental principles for deriving interpretations. One of these is *the principle of similarity*. A second one is *the psychoanalytic standard operation procedure*, which was quoted in Q-163:1. A third one is *the illusion of separation*. Two further principles are particularly prominent to the interpretation at hand: *the principle of prestige* and *the postulate of the outgroup*.

**THE ILLUSION OF SEPARATION:** *Suppose you want to find the causal event that was responsible for a given phenomenon F. Throw a glance at the complex situation. If you should notice any event or circumstance which MIGHT be the cause of F, take for granted that it is conclusively proved that the latter is really the cause. Furthermore, take for granted that all other causal relations Which might just as well have been responsible for F, are completely non-existent.*

**THE PRINCIPLE OF PRESTIGE:** *An interpretation should be so selected that the prestige of the psychoanalyst will be increased/enhanced and/or the prestige of the patient will be reduced.*

**THE POSTULATE OF THE OUTGROUP:** *The ingroup consists of the psychoanalysts and the successfully psychoanalyzed individuals. Psychoanalytic theory is postulated to be valid solely for the outgroup. [Q-186:1]*

From *the postulate of the outgroup* the conclusion follows that Freud could not possibly have manifested such reactions as rationalization and projection in relation to Dora. The same conclusion may be derived from *the principle of prestige*.

§ 187. There would be no reason to make much fuss about a patient dropping out of treatment. But I have already noted that Freud almost suffered a nervous breakdown, and saw a gigantic insult in this behaviour of Dora's—an insult which Freud *15 months later* [!] FORGAVE [!] Dora. He re-interpreted her dropping out as a *pathological* reaction; as an escape from *her own* feelings; and moreover, from feelings of *love of Freud*; as a reaction she herself *did not understand*, so that she was actually *a marionette in the hands of her own unconscious*; while Freud *saw through her*. And this set of interpretations will automatically decrease Dora's prestige and increase Freud's prestige.

In other words, Freud may help himself to "get even" after his pathological

feeling of having lost a considerable amount of prestige, by means of distortion of reality.

In Q-115:1, ex.11, we encountered Felix Deutsch' transformation of an interpretation into an observation, viz. the unconscious thought attributed to Dora by Freud (GW-V:283f./SE-VII:119f.): "Men are all so detestable that I would rather not marry. This is my revenge." It is precisely this alleged thought of hers that is part of the consolation lie just described. Dora allegedly revenged herself on Freud by dropping out, in the same way in which she obtained revenge upon Mr.K. by giving him a slap in his face and leaving him when he tried to seduce her.

§188. I shall show how Loch's theory will combine with Freud's interpretations about Dora.

It was not really the case that Dora was in love with Mr.K. when the seduction attempt occurred. Her psychoanalyst merely constructed this fiction (consolation lie), because the belief in her own love would 1. enable Dora to make sense of things, 2. enable her to go on living, 3. free her from any fixation, 4. open possibilities to her, and 5. enable her to discover and construct new meanings.

Likewise, Dora did not fear her own love; nor did she try to escape from her own feelings at the seduction attempt.

However, 30 months later she really loved Freud, really feared her love, and really escaped from her own feelings. And this second love, fear, and escape reaction were *replications* of the love, fear, and escape reaction she *really* felt at the seduction attempt.

A few other combinations of Loch's theory with Freud's case-study, are possible. E.g. this. Dora did not really love Freud, nor did she fear and escape from: this love. But her transference was no less real. She *really* transferred from one fiction to another.

The least comprehensible version will emerge, if both the first love etc., and the second love etc., and also the "transference", are conceived of as fictions.

Brutal or sophisticated attempts to make a patient believe such things, would lead to the very opposite result of consolation. There is reason to fear what might have happened to Dora as a consequence of Freud's treatment, if she had not been in the possession of a strong personality.

§189. One of the writers to whom I am strongly indebted is Frank Cioffi, who points out that:

"There are a host of peculiarities of psychoanalytic theory and practice which are apparently gratuitous and unrelated, but which can be understood when once they are seen as manifestations of the same impulse: the need to avoid refutation." (Cioffi, 1970:473) [Q-189:1]

Loch's re-interpretation has unambiguously made psychoanalytic theory immune to any straightforward attempt at refutation. Should any critic point out that this or that interpretation is flagrantly incorrect, psychoanalysts may retort: We have never said that psychoanalytic interpretations are *true inferences*. According to

our view they are not even *tentative inferences*. They are nothing but *pure fictions* invented for the purpose of consoling the patient. [In other words, psychoanalysis is a most primitive version of suggestive therapy, aimed at nothing more than soothing the patient.]

But still in 1973 Sjöbäck could rightly state:

"Undeniably, the concept 'defensive process' is crucial to the theory as a whole. Without it the entire edifice would collapse." (Sjöbäck. 1973:1) [Q-189:2]

§190. It may be worthwhile to compare Loch's theory with other recent attempts at saving Freud. Different attempts are flagrantly incompatible.

A sexually impotent young male may have undergone psychoanalytic treatment for 6 years without any change. He may from the very start have made clear that his sole aim with undergoing this variety of treatment is to be cured of this symptom. He may at the very beginning have asked the question whether psychoanalysis will cure his condition. He may have got an answer which he has taken as affirmative.

[The psychoanalyst may afterwards deny that he has given any affirmative answer. The tape-recorded dialogue may reveal that his actual answer to the patient's question had the following wording: "Well, helping people with their sexual troubles is the special domain of psychoanalysis." That is, he deliberately constructed a formulation which most people would take as a straightforward affirmative answer—but a formulation with a built-in "fire-escape", so that the psychoanalyst had prepared for the "justification" that he never said anything of the kind.]

After 6 years the patient may complain of the lack of effect of the treatment. His complaint may be met with an interpretation.

*Orthodox Freudian interpretation*: "You have not recovered because unconsciously you want to remain impotent."

*Orthodox Freudian theory*: The interpretation is literally true in the scientific sense of the word.

*Neo-Freudian interpretation*: "You expect someone else to do the job for you. But you will not recover until you learn to take responsibility for yourself." (This is just a re-phrase of the orthodox Freudian interpretation.)

*Neo-Freudian theory (ego psychology)*: The interpretation is literally true; and so is the orthodox Freudian interpretation.

*Hermeneutic interpretation (according to the version presented by Lesche & Stjernholm Madsen, 1976)*: "But you could at any time have changed your condition just as easily as you could change dinner-time. You still can do so."

*Hermeneutic theory (in L&SM's version)*: The hermeneutic interpretation is literally true.

*Interpretation according to Loch's re-interpretation*: (Any of the above interpretations may be given, or others of a more or less different type.)

*Loch's theory*: These interpretations are consolation lies which will make sense to the impotent male, enable him to go on living, and open possibilities to him.

[Q-190:1]

Should Loch try to escape by means of the idea that he meant to suggest that interpretations of a *radically different* nature should henceforth be constructed and delivered, he will find himself in a strange position: psychoanalytic theory is false, psychoanalytic interpretations are false, the previous therapeutic behaviour of psychoanalysts should be substituted with something of an altogether discrepant nature. Why then retain *the name* of psychoanalysis?

§191. One of the most basic peculiarities of psychoanalysis, is the diminutive amount of change that has occurred during almost a century. Recent psychoanalysts and their adherents are in the habit of a few marginal details of little significance, retaining all the fundamental constituents of their theory and method, and inventing a new name. And then they claim that any criticism directed against any previous variant, is automatically beside the mark.

## Second Book

The Clinical Evidence  
of the Seduction Theory:

*A creatio ex nihilo*



## Chapter 11

# Preamble

*Discoveries of any great moment in mathematics and other disciplines, once they are discovered, are seen to be extremely simple and obvious, and make everybody, including their discoverer, appear foolish for not having discovered them before.*

G. Spencer Brown

*Was ist das schwerste von allem? Was dir das leichteste  
dünket:*

*Mit den Augen zu sehen, was vor den Augen dir liegt.*  
Friedrich Schiller

§192. Since the very first beginning of anything worthy of the name of a behavioural *science*, no other theory has ever held a position even remotely similar to that of psychoanalysis. One might easily get the impression that Freud did for psychology and psychiatry what Newton did for physics. And just like Newton's contribution has crucial implications for astronomy, geology and biology etc., so Freud's contributions would seem to have significant implications for sociology, anthropology, the science of education, and other related disciplines. Let us make a brief survey of the influence of psychoanalysis.

All over the Western world, the student of psychology will usually encounter Freud's theory within many subfields: psychology of learning, of cognition, of motivation, of social relations, of maladjustment, of personality, of education, and so on. At the very least, psychoanalysis will be presented as one of the very few topmost primary theories. Not infrequently psychoanalysis will even be presented as the only mature psychological theory.

But not only the evaluation of the theory is remarkable. As regards all other theories the same pattern will re-occur. 1. The nature of the theory will be explained. 2. A set of observations will be listed which is thought to support the theory, or which have been discovered by means of the theory. 3. A number of objections and (at least *prima facie*) embarrassing data will be mentioned.

By contrast, the student will never be informed about any fact that is embarrassing to psychoanalysis. And still in the 1970s he might have read psychological courses for more than 10 years without having ever encountered any single objection to this theory.

§193. The science of education may be presented in basically two different ways, viz. as a kind of applied psychology, and as an independent discipline. In

the former case, the just described peculiarities of psychology may be mechanically carried over to the "affiliated" discipline. In the latter case, the student will frequently have to learn a number of applications of psychoanalysis to the areas of upbringing, teaching, classroom management etc.

Sociology may be presented in many ways. Some of these bear little relation to psychology. Among the others, psychoanalysis will very often occupy the primary place. And this is so, despite the fact that a series of other psychological theories are conspicuously much more congruent with the field of sociological inquiry.

Social anthropology is still struggling to assert itself as a truly independent discipline. Many anthropologists are therefore strongly opposed to the application of *any* theory borrowed from *any* other discipline. At the same time, those anthropologists who are prepared to borrow non-anthropological theories, will usually take either psychoanalysis or Marxism as their first choice.

§194. Within psychiatry, the influence of psychoanalysis is at present even greater than within psychology. And exactly at the time I am writing these lines, this influence is increasing.

The field of emotional and behavioural disturbances is distributed over psychological, psychiatric, and education departments. But both the theoretical outlook and the practical interventions are thoroughly based on the theories of Freud and his followers. Only recently have radically different approaches become—*not recognized*, but merely *tolerated*, and only as *marginal phenomena*.

A certain influence within criminology may likewise be traced.

Extrapolations of psychoanalysis to non-behavioural fields are legio. A few such fields are the disciplines concerned with literary criticism, art, music, movies, politics, jurisprudence, religion, philosophy.

§195. However much the academic community has been overwhelmed by psychoanalytic views, the lay community has been so to an even greater extent. Novellists, movie directors, journalists, TV reporter, and many others, are incessantly propagating psychoanalytic propositions as firmly established truths, while ridiculing opponents of their favourite ideology. They are disseminating the most horrible desinformation about the merits of psychoanalysis and the alleged flaws and irresponsibility of (e.g.) behaviour therapists. Mass media people may have the opportunity of stopping criticism of psychoanalysis and will, more often than not, misuse this opportunity.

A state of things recurrently observed throughout history is the phenomenon of prevalent lay conceptions being the reflexion of prevalent academic conceptions. But in the present context this pattern was reversed. Psychoanalytic theories became widespread throughout the lay community *before* they became so within the academic community. Academicians could not possibly have come to accept psychoanalysis because they had been exposed to the same kind of circumstances which had lead them to accept any other theory. They were never exposed to any empirical observations, nor to any defensible derivations.

I suggest that many academicians came to accept psychoanalytic theory be-

cause of one and only one reason: the idea occurred to them that this theory *must* be true, since it is incessantly presented *in the lay world* as being true.

This is not an attempt of shifting the blame away from the academic community. Quite the opposite thing is true. It is no flattering suggestion that academicians were passive victims of primitive propaganda devices.

§196. Anyway, psychoanalysts have from the very start turned directly to the lay community. They had an easy match. Fascinating novels, plays and movies, as well as seducing newspaper reports may be inspired by psychoanalysis, while most non-psychodynamic theories will look rather pale in comparison.

Consequently, fictional writers and journalists became the true decision makers. For generations, the therapeutic approach was not selected on the basis of any considerations as to which approach will be most efficacious in curing sick and severely tormented people—but on the basis of the evaluation of the efficacy of inspiring an interesting plot in a novel.

§197. If I have presented some of the pieces of information in the wrong order, I shall remedy this imperfection now.

In view of its powerful position, one would think that psychoanalysis must be strongly backed by a very comprehensive body of firmly established empirical observations. Amazingly as it may seem, the very opposite is the case.

If a psychoanalyst is asked why he thinks his theory is true, he will absolutely never invoke any body of actual observations (whether from his own clinical practice or from the published literature), and claim that these observations prove or support the theory. Neither will this option ever be chosen by university teachers and other proponents who are not practicing clinicians.

Orally as well as in print most of my readers might have encountered an abundance of postulations concerning the prominent features of the very observations which can be found in the *published* writings. Their sheer number is said to be immense, both per se and in comparison with the amount of data found in non-psychodynamic writings. Their nature is said to be highly surprising and unexpected. Their evidential power is said to be absolutely conclusive and, moreover, to exceed the power of the support of any non-psychodynamic theory.

We should perhaps not be surprised at the postulations of psychoanalytic propagandists. But numerous university teachers who cannot be suspected of having illegitimate motives, have uncritically repeated the very same postulations.

§198. Even the most cursory glance at the literature will reveal that the observations are exceedingly few in number; they are almost invariably trivial and shallow; and they have hardly ever any evidential power.

The case-study of Dora contains a total of one non-trivial observation claim, viz. the postulation that Dora recalled the spying event after Freud had succeeded in lifting the repression. This is also the sole observation claim providing any support for any psychoanalytic proposition. And we have seen that exactly this datum is faked (cf. §§17).

In the present volume we shall see that all instances of de-repression postulated in the third seduction paper of 1896, are likewise faked.

All 17 volumes of *Gesammelte Werke* written during 46 years, contain much less than 17 instances of postulations (not even descriptions) of concrete instances of de-repression. The majority of these can be *directly* proved to be deliberate fabrications. Common sense would hardly allow for the suggestion that the remaining published instances might be authentic, and that Freud told the truth, whenever he claimed to have encountered a wealth of instances of de-repression he regularly encountered in his own practice.

In the present case we do not have to rely on the dictate of common sense. In a later volume I shall describe the exact procedure by means of which we may, assisted by Trankell's (1971) *criterion of isomorphy*, generalize and *prove* that all instances of de-repression (e.g. the account of the lady with the stain on the tablecloth, in *Vorlesungen*), are frauds. Nonetheless, we shall not yet be concerned with *all* instances of postulated de-repression.

§199. In one respect I shall qualify my claim about the complete absence of non-trivial observations throughout Freud's writings. The rat man was an unusually interesting individual. Consequently, even an untrained and not very gifted lay observer could hardly fail to produce an interesting description. But Freud's own description does not contain *any* feature of this patient, which a journalist, a lawyer or a physicist might not have described at least as satisfactorily,

§200. At the present time it is a well-known fact in no need of any documentary support, that psychoanalysis never met much resistance. Nothing can be found during any period, which is even remotely akin to the furious attacks and secret intrigues directed against experimental psychology and behaviour therapy during generations. There is no historical truth behind the myth about the hostility with which Freud was met. As for the alleged 10-year-period of isolation from his profession, the experts cannot even agree on its location in time. Ford & Urban (1963:112) place it at 1890–1900. Masson's (1984) indication is 1896–1906.

Many causal factors may be responsible for the scarcity of critical papers. I would dare neither to construct an exhaustive list, nor to make any guess as to the relative contribution of each. A few factors may be mentioned. Psychoanalysts have always applied the most intensive propaganda techniques. Any researcher would feel that his reputation would be endangered if he openly attacked Freud. Proponents holding key-positions may have managed to stop a non-neglectible number of research reports. The possibility of irrational factors should not be overlooked. Repeatedly during history, both academic sciences and areas of lay thinking have been overwhelmed by flagrantly false ideas. Frequently during such periods even *the opponents* of the prevalent set of ideas have been as blind as the proponents, of the most crucial and transparent defects of the view they have attacked.

Psychoanalysts have always asserted that the views held by *non-psychoanalyzed* individuals, is to a considerable extent caused by irrational factors related to the *individual* level, and these factors are said to explain why some people *do not believe* in psychoanalysis. Obviously, there is a much greater need for identi-

fying *social* factors responsible for the fact that other people *do believe* in psychoanalysis.

§201. A limited number of critical papers have been produced now and then. Unfortunately, the discipline of psychology has a short memory. Today, almost all critical investigations published before the second world war are forgotten—despite the fact that many valid and crucial objections were raised in these papers, and have never been answered.

Whenever psychoanalysts had any choice, they ignored any criticism. And this is an effective strategy for ensuring that the criticism will soon be forgotten.

I shall list or discuss only a few papers which, according to my view, are particularly important. (I might have neglected certain papers that might be equally important.) In 1934 the Finnish anthropologist Edvard Westermarck produced a study with the title *Freud's Theory of the Oedipus Complex*. Unfortunately, the book was published in Swedish. But it would be well worthy of being re-edited today in English.

A prominent place must be given to the contributions by Hans-Jürgen Eysenck's (1952, 1960b, 1960c, 1964b, 1964c) studies of the effect and nature of therapeutic intervention and related aspects. Other papers by Eysenck (1957, 1956) are intended as a kind of popular scientific reading. But we should not therefore overlook the fact that they contain many highly sophisticated analyses of very significant issues.

Eysenck & Wilson's (1973) *The Experimental Study of Freudian Theories* is a most essential dissection of a series of experiments to which proponents of psychoanalysis have attributed a particularly strong evidential power. Eysenck & Wilson have shown, in *my* words, that each and all positive outcomes have been produced by means of a small set of very primitive techniques for manufacturing evidence where there is none.

Two studies of the effect of numerous varieties of psychological therapy, Rachman (1971), 2nd. ed. Rachman & Wilson (1980), have already come to be conceived of as standard handbooks which it is not permissible to ignore.

§202. Grünbaum (1984) has obtained a comparable status. Any brief description of the content of this book may be misleading. I may only hope not to do too sizable an injustice to this writer. His refutation of the hermeneutic re-interpretation of psychoanalysis, is for most purposes preferable to my account in §§168–173. I have not proved anything, but have merely given a popular account which many readers may more easily follow.—Having shown that the hermeneutic re-interpretation is untenable, Grünbaum returns to the “normal” psychoanalytic theory. The primary focus is on the evidential support of the theory.

Grünbaum is at the same time more and less generous than I am. I am somewhat more tolerant of internal logical defects *at higher theoretical levels*. Grünbaum is much more tolerant of strange observation claims. He really gives Freud every opportunity to show himself from his most favourable side.

§203. A hypothetical example. If confronted with the spying event, Grünbaum would take at face value Freud's statement that Dora had really recounted



the event and had really claimed that she had a genuine recollection of the event. Grünbaum would likewise take for granted that Freud was in good faith when he stated his conviction that he had applied no suggestive influence to Dora and, in fact, was in the possession of a method which enabled him to distinguish with certainty the effect of suggestion from authentic recollections and, hence, enabled him to rule out the possibility that the spying event derived from suggestion. Grünbaum would in turn perform a meticulous analysis and show that Freud's view is false: it is *in principle* impossible to rule out the suggestion hypothesis.

For better or worse, I would rather focus on Freud's observation claims themselves. By means of a textual analysis I would point out that Dora never recounted the spying event; that Freud was perfectly aware of the fact that he himself had fabricated the event and put it into Dora's mouth; that Freud incessantly applied the most coarse and brutal persuasive techniques to induce his patients to accept his postulations about what happened during their childhood or infancy; and that he was perfectly aware of not telling the truth, when he claimed that suggestion was absent from his consultation room.

§204. *Digression.* A number of philosophers, e.g. Grünbaum, Glymour, Toulmin, Scriven and Sherwood, have attributed methodological sophistication to Freud. I fail to find any trace of this property in *Gesammelte Werke*, and the instances selected by these philosophers are unconvincing. Because of the high quality of Grünbaum's contributions, I shall discuss one of his illustrative examples.

Freud produced his writings in a community in which numerous hypnotherapists would implant false memories into their patient, thereby sometimes achieving symptom removal. The entire profession was very much aware of the fact that hypnotized individuals have a most remarkable capacity for perceiving the therapist's unspoken thoughts. Almost any psychiatrist would spontaneously guess that Freud might, unintentionally and unawares, have exercised some suggestive influence; that his patients had somehow perceived what he expected from them, and had fabricated in order to please him. Hence, Freud's colleagues would arrive at the verdict that his theory had obtained no support from the patient accounts.

Very few people would not have realized that Freud had only one option. He must assert the absolute absence of any kind of suggestive influence. And he must assert that he had developed a valid method of verifying the absence of such influence.

With only trivial modifications, the same argument may be applied to every situation where there is *prima facie* room for the attribution of methodological sophistication to Freud.

§205. Very few papers have asked questions about the nature of the observations actually invoked by Freud, except as an adjunct to the solution of other problems. But one such paper exists, and despite its brevity it is the most important study of psychoanalysis ever undertaken, viz. Joseph Wolpe & Stanley Rachman's *Psychoanalytic 'Evidence': A Critique Based on Freud's Case of Little*



*Hans* (1960, reprinted 1963 and 1964). The fact that this paper is so crucial for anyone intending to learn anything about the nature of Freud's theory, does not only derive from the keenness with which these writers have pursued their analysis; but also from their choice of subject. I wish to emphasize a point that is often forgotten, and perhaps more often so by philosophers than by empirical scientists.

Suppose there exists a comprehensive body of highly unexpected and most remarkable observations (O:1), which are in dire need of an explanation. Suppose further that only one theory (T:1) has so far been invented, which could do anything in the way of explaining O:1. In this situation it might be important to know, say, that T:1 is flawed by internal contradictions and other defects. Nonetheless, we might well decide to retain T:1, if our only alternative would be to face a chaos of enigmatic data.

On the other hand, suppose that no observations related to T:1 exist. Nor is there any reason to believe that we would ever discover any such observations, whether or not our research be guided by T:1. In this situation we would reject T:1, however free from any flaw it might be. In other words, the nature of the empirical backing takes priority over the theoretical features.

§206. It is a legitimate task to study the internal structure and properties of any theory—astrology, renaissance theories of witchcraft, Einstein's theory of relativity, and so on—in complete isolation from any considerations of the empirical backing of the theory. No researcher should be criticized for studying psychoanalysis in this way—note! *if he is aware of what he is doing*.

But to my considerable knowledge, no writer has ever restricted his investigation of psychoanalysis to the internal theoretical structure, while it was his view that neither the truth nor the importance of his results would need to be seen in a different light, in case it turned out that no empirical support of psychoanalysis exists.

What I *have* found, is studies based on the assumption that there is no need to pay much attention to the clinical basis, because the latter is already known to be rather satisfactory. It may be fairly obvious that the writers would never have considered their undertaking worthwhile, if they had not believed in this assumption.

In other words, even the writers who pay much attention to theory and little attention to observations, share my view that the observations are primary, while theory is secondary.

§207. All the above mentioned studies by Eysenck, Rachman, Grünbaum, G.I.D. Wilson and G.T. Wilson strongly focus on the empirical backing. But none of them has given as prominent place to the clinical observations as Wolpe & Rachman (1960). Strangely, after many years of study of clinical psychopathology, numerous students have not even learned about the existence of this paper. Moreover, no proponent of psychoanalysis has ever tried to find any error in Wolpe & Rachman's study. (I hope I shall escape the objection that this is exactly what Edel, 1968, has done.)

§208. Psychoanalysis is so fragile that it will collapse, entirely and irremediably, regardless of what specific aspect is selected for criticism. Later, I shall list more than 30 possible lines of attack. And I welcome contributions along any of them. But I regret the fact that so little interest has been devoted to the investigation of the clinical observations invoked by Freud himself. So far, Wolpe & Rachman (1960) seems to be the only study based on a textual analysis of the clinical observations.

Basically, *The Non-Authentic Nature of Freud's Observations* is a replication and extension of *Psychoanalytic 'Evidence': A Critique Based on Freud's Case of Little Hans*. I shall occasionally comment upon the psychoanalytic theory. I shall not infrequently ask whether the latter is actually supported by the observations claimed to prove it. But *my central task is to ask questions about the observations themselves: what is the nature of the data found throughout the writings by Freud and his followers?*

§209. Already at this point a misunderstanding may creep in. There exist a considerable number of papers in which half a dozen techniques of philosophical analysis are applied. Many of these techniques have impressive names; but whatever their proper merits, they are in these papers applied for the purpose of confusing rather than illuminating the issue. The philosophical or methodological commentator may pay no attention at all to any datum found in Freud's writings; he may merely advance the dogmatic assertion that Freud did so or so. A complex apparatus may well be eminently suited for concealing the crucial factors.

I shall list a few specimens of answers of a type I shall definitely not give.

"Freud's condensation and displacement were equivalent to the syntagmatic and paradigmatic axes of language, the poles of combination and selection—and equivalent as well to metonymy and metaphor in poetics and contiguity and similarity disorders in aphasia. [...]"

For Freud, free association constituted an orderly linguistic analysis of the route from an underlying, repressed content to its distorted superficial manifestation. This is much like what transformational grammarians 50 years later would call the recovery of deletions between deep and surface structures." (Bruss, 1976:122, 127) [Q-209:1]

"The mode in which Freud wrote [...] along with its processiveness, it is eminently concordant, seeking to establish, now a commonness, now a complementarity, between disparate entities and realms of experience. In uniting different time frames in the present, Freud's multitrack stereophonic expression is distinctive." (Mahony, 1986: 222) [Q-209:2]

"Upon completion of the treatment, the patient comes into possession of his true biography. [...] A humanistic theme of self-knowledge enters the medical endeavors." (Bergmann, 1976:23) [Q-209:3]

§210. My approach is exceedingly simple and based on little more than common sense. The four juxtapositions (Q-14:1 etc.) supply an excellent illustration of my procedure. My first question is: *What kinds of observations are found in Freud's writings?* And typical answers of mine will be: A gouvernante's hat blew off while she was taking a ride. Children may defecate in their pants.

Further typical questions: *Are Freud's observation claims non-contradictory?*

The macrophysical reality is evidently non-contradictory. Hence, one or more contradictions between a set of observation statements prove that the set is not in agreement with reality.

*Is any single observation non-trivial?*

*Is any pattern of observations non-trivial?*

*Are the observations esoteric in some respects; or are they rather of such a nature that they can easily be adequately presented in print without any non-trivial loss of information?*

§211. The last question is by no means trifling even if asked about observations which have already appeared in print. *The structure and properties of the body of public observations may well throw much light upon the structure and properties of the non-published observations.* They may do so in a variety of ways. And I shall devote some space to the discussion of this problem, since inapposite objections are likely to turn up.

Dora was in a depressed mood at her 18-year birthday, despite the fact that she got many expensive presents. Freud asserts the explanation that she missed a present from "the beloved" Mr. K. I shall disregard the fact that she most probably was *content* because Mr. K. had not intruded at that occasion. But her birthday occurred 24 days after the start of her psychoanalytic treatment. Recall that she, who had a strong will, had been *forced* to submit to a treatment aimed at teaching her that the seduction attempt had only taken place in her own highly-strung imagination.

Most people would have been surprised if Dora had been happy under these circumstances, whether or not she had received any present from Mr. K. But Freud overlooks the possibility that the gigantic and prolonged insult Dora had had to stand, could have had any effect upon her mood.

Once more, Freud has demonstrated his inability to detect features of the situation, which would have been conspicuous to most lay men. One need not be a textual analyst in order to realize the impossibility that the secret observations gathered by the same clinician were much more fine-grained than the observations made by non-psychodynamic psychologists.

So many specimens of Freud's interpretations have already been cited throughout the present report, that it is clear that the example about Dora's birthday mood is *paradigmatic*.

§212. In §§145–156 I cited Freud's own postulations about the three kinds of proofs of the validity of psychoanalytic interpretations. It is seen at a glance that all three categories are of such a nature that they could most easily and without any loss of information be rendered in print. The same is true of other varieties of proofs mentioned in passing by Freud, but never given the status of fundamental and encompassing validation procedures. Later, we shall encounter the postulation that a crucial part of the proof of the seduction theory consists of the convergence of different lines of free associations. And Rapaport (1965:118) claims that interpretations may be proved by the patient's (subsequent) associations. It is apparent that both Freud and Rapaport are talking of verbal strings, not of tone of

voice and such things—hence, of evidence that could hardly be more suited for appearing in print.

§213. Until the 1980s, Freud and his followers agreed unanimously upon the view that the published writings contain so powerful evidence, that anyone who is not convinced of the truth of psychoanalysis after having read such writings, is necessarily a victim of a pathological will to distort reality. I shall present only one testimony about the nature of the *published* evidence:

“Can I expect you to believe this extremely important thesis [viz. that all neurotic symptoms are caused by specific sexual factors] on the evidence of two examples? No. [...] So I must be content with having given a specimen of the support of my assertion and, for the rest, I refer you to the reports given in the literature of the subject—to [...] all the other papers that have since then filled our periodicals. There has been no lack of investigations precisely on these lines.” (GW–XI:277f./SE–XVI:269f.; James Strachey’s translation has been corrected) [Q-213:1]

But after prolonged attack by a competent critic, the very same psychoanalysts may make a volte-face and shift to the position that it is an inescapable feature of psychoanalytic observations that they cannot be rendered in print.

When psychoanalysts or their proponents have rebuked the critic by means of this fabrication, they will usually return to their previous position, and once more accuse all skepticists of suffering from pathological blindness. I shall cite concrete examples in the volume on the psychoanalysts’ persuasive techniques.

Once more, it would be a far-fetched hypothesis that persons who apply such *strategic oscillation*, are trustworthy when they make postulations about the nature and evidential power of their *secret* observations.

§214. I have to some extent already answered the next question. *Are psychoanalytic observations unusually fine-grained? Do Freud and his followers pay close attention to highly sophisticated aspects of the clinical reality, or are they solely interested in the most coarse aspects of the observables?*

*Are the observed events and features esoteric in the sense that they could only have emerged under highly specific observation conditions, viz. in the psychoanalytic situation? And will they be perceptible only to a person who has undergone a prolonged and highly specific variety of training?*

To give a concrete example: would a non-psychoanalyst unaided by the psychoanalytic situation have no chance of discovering that Dora did not receive a present from Mr. K. at her 18-year birthday?

§215. *When Freud or any of his followers present both an interpretation and the set of observations claimed to prove the interpretation: does this set consist of an infinity of observations, or of merely some 1–5 observation?*

I shall also ask the question: *Is the inference valid: does the set of observations really prove the interpretation?*

Occasionally, I shall devote some attention to the following problem: *What pattern of observations could at all prove the theory or interpretation at hand?* This problem may be very important. It may turn out that no pattern of any evidential power could possibly exist. Consequently, Freud *could not* have been in

the possession of water-proof secret evidence. And only one concomittant question remains: was Freud incompetent as a methodologist, being unable to perform even the most elementary evaluation of his secret data—or was he deliberately bluffing, knowing perfectly well that he had no secret data at all?

Freud and Alice Miller claim to have collected an abundance of observations which prove the seduction theory. Jeffrey Masson has avouched that Freud was correct. This is a situation in which numerous readers will give no thought to the question how such a pattern could possibly look; they may mechanically fancy, well, why shouldn't these psychoanalysts have discovered such a pattern?

§216. But I did not leave things at that. And however much Freud, Miller and Masson have postulated the real existence of a configuration of genuine evidential power, they would very likely be highly surprised by learning that such a configuration is not formally impossible. In fact, I shall in §§376ff. construct a pattern of observations about a fictive patient, Victor, which, if it should ever be encountered in the empirical world, would indeed prove Freud's seduction theory. This is no little achievement—in particular, because some reflexion will reveal that the seduction theory could not be verified by any pattern widely at variance with the one I have constructed.

Only one thing is wrong about this biography. It is hardly possible that any psychoanalyst at all would be prepared to believe that *this* pattern could ever be encountered in the empirical world.

In fact, psychoanalysis is so curious a theory, that the following feature may be found over and over again. *Confirmatory observations are as embarrassing to psychoanalytic theory, as disconfirmatory data.*

This is no paradoxal statement. Disconfirmatory data are embarrassing, because of the simple reason that they are incompatible with the theory.

But it will turn out that: a configuration could only be confirmatory, if it is of such an extremely unusual kind that no one is capable of believing in its possible empirical existence. There may be no *formal* obstacle as to why it could not be found. But human beings rightly refuse to believe in the *real* possibility of many things that are *formally* possible.

It is not legitimate to discuss such matters in the abstract—e.g. to raise objections like the following one. Psychoanalysis is an extremely unusual kind of theory; then why shouldn't the observations from which it is derived, likewise be of an extremely unusual kind?

The reasoning should be tied to the concrete world. One should look at one particular configuration of observations at the time and ask oneself: is exactly THIS configuration a pattern I am prepared to accept as being *empirically possible*?

§217. A further question is this. *Are the configurations of observations postulated by Freud reasonable, believable?* It is an altogether irrelevant fact that the most unbelievable circumstances have eventually turned out to be true within advanced physics. Freud's writings are replete with observation claims that could hardly be more easily testable. His followers have for more than half a century felt reluctant to mention these. And experimentalists who are prepared to apply bold acrobatics to derive testable propositions from psychoanalytic theory, delib-



erately shun all easily testable observation claims. And for good reason. Here is a specimen.

"I believe there can be no difficulty in substantiating the statement that infants only soil with their excrement people whom they know and are fond of; they do not consider strangers worthy of this distinction." (GW-XII:113n/SE-XVII:81n) [Q-217:1]

Some readers may spontaneously have noticed the similarity between this quotation and Q-3:1 by Winnicott.

§218. It is an important fact that Freud's non-trivial observations have no foundation in the clinical reality pertaining in his consultation room. However, I shall take one further step and ask, *what causal factors were responsible for the falsity of Freud's observation statements?*

Many volumes could be written about the infinity of errors deriving from carelessness in *Gesammelte Werke*. But I do not think this feature belongs to the most crucial ones, although a few aspects are remarkable. First, Freud's followers have repeatedly testified on the absolute consistency of Freud's observation claims (cf. Q-31:1, Q-139:3).

Second, some of the specific effects of carelessness may provide unexpected information about Freud's personality, cf. ch.3. Interpretations, which have for a century been taken to prove Freud's his sublime insight into the minds of his patients, apparently derived from such a primitive and "mindless" phenomenon as *perseveration* in himself.

§219. Among all my results, none is more original than the claim that *Freud has deliberately fabricated all his non-trivial observations. Some of the latter have been fabricated out of nothing, while others have been produced by distortion of entirely different authentic events—so that they could be used or misused to support psychoanalytic interpretations or theories. Likewise, Freud has deliberately suppressed and concealed a non-neglectible number of observations, because he was perfectly aware that few readers would be prepared to accept his interpretations, if he had presented these observations.*

I have not formulated the last theme as a question. But there are hardly more than three ways of reading Freud's writings for even half an hour, without noticing that Freud incessantly fabulates and that he has never discovered anything at all. Either, one must have a very short memory, so as to escape comparing statements found on adjacent pages. Or one must suppress his critical capacity. Or, whenever Freud said something particularly inadequate, one must assume that he really meant something else.

Some of he techniques of concealing, distorting or fabricating observations, could hardly be more transparanet. Others are rather opaque, and may be exposed only by means of a complex textual analysis.

§220. The above questions and themes are intended to give a general impression of the specific character of my approach. The list is not exhaustive, not even as regards the two volumes published now. Moreover, in later volumes I shall ask questions and analyze themes of a rather different nature. I shall study *the proce-*



dures and criteria by means of which we may distinguish deliberate untruths from other kinds of errors. One such criterion was presented in ch.9. And I shall eventually present an atlas of Freud's persuasive techniques.

I shall now return to Wolpe & Rachman (1960). At the first glance it might seem as if I had considerably extended the methodological approach developed by these writers. But apart from my strong emphasis upon the phenomenon of deliberately fabricated observations, I do not think this is really so. The implicit riches of the re-analysis of the case-study of Little Hans, are at least as valuable as the explicitly stated results. I shall list some of the implicit conclusions.

First, Freud did not in the least "observe the unconscious". Nor did he discover any new kinds of data. The nature of his data does not differ from the nature of pre-analytic data. Second, Freud did neither discover any new patterns of data. Third, his sets of data are by no means extensive and comprehensive. Instead, he merely picked up a few isolated details here and there, on the ground that they could be used or misused to back up some interpretation or another. Fourth, unconscious phenomena are asserted solely as inferences; nowhere is any observation said to contain any admixture from the unconscious. Fifth, not one single inference is convincing. The postulated unconscious phenomena are taken from Freud's own preconceived prejudices, and then "deduced" from trivial observations by means of *the principle of similarity* (" 'A is similar to B' implies that 'A is the cause of B' "). Sixth, it is simply not true that each interpretation is based on innumerable many observations. On the contrary, each detail is interpreted in isolation and with little regard for other details. Consequently, the whole picture is fragmentary and incoherent, and contains glaring inconsistencies.

Implicitly, Wolpe & Rachman have already answered almost all my questions. In the case-study, Freud claims to have proved the reality of the Oedipus complex, castration anxiety, displacement of emotion, and various other fundamental concept. But Wolpe & Rachman reveal that Freud has mechanically imputed these doctrines upon a number of trivial data which, on the one hand, are devoid of any evidential power whatever and, on the other hand, would be directly available to any layman.

Wolpe & Rachman have indeed pointed out a few inconsistencies between Freud's observation claims. But on the whole they have been exceedingly generous. The observations and interpretations used for re-analysis have been so selected, that Freud is given the greatest possible opportunity of proving his claims.

§221. Wolpe & Rachman's re-analysis of the case-study of Little Hans have likewise been misrepresented: as a controversy between psychoanalysis and learning theory. The validity of the re-analysis is said to be entirely dependent upon two assumptions: that learning theory is known to be true; and that its application to cases like that of Little Hans is known to be true.

In actual fact, a very small section of Wolpe & Rachman's paper has anything to do with learning theory. And even this section merely apply such elementary aspects of learning theory, as were widely accepted centuries before Pavlov,

Bekhterev and Watson. Flagrantly, the re-analysis would remain valid, even if it should turn out that learning theory is altogether false.

§222. Both these researchers hoped that other psychologists would be prompted to re-analyze other psychoanalytic writings. This hope was not fulfilled during 30 years. But I think that dozens or hundreds of behavioural scientists would have accepted the challenge, if they had realized how extraordinarily profitable a textual analysis may be.

## The Problem of This Report

*Mais on arrange aisément les récits du passé que  
personne ne connaît plus, comme ceux des voyages  
dans les pays où personne n'est jamais allé.*

Marcel Proust

§223. In the early 1980s the whole academic and lay world was befallen by an enormous interest in Freud's seduction theory. But without any exception I have been able to find, the commentators demonstrated their inability to perceive the content of Freud's text. I postponed my ongoing research on the case-study of Dora, and set out to put things right.

There are three or four views of the seduction theory. According to the report read to *Verein für Psychiatrie und Neurologie in Wien* on 21 April 1896 (= the third seduction paper, GW-I:425–459/SE-III:189–221), Freud had succeeded in curing 18 hysterical patients of each and all their symptoms. It had turned out that each and all patients had been sexually seduced during early childhood, some of them by three or more different persons. They had completely repressed the events. By means of a specific innovative therapeutic technique, Freud had succeeded in lifting the repression for all seduction events. And de-repression had invariably lead to symptom removal. Moreover, Freud had performed the most meticulous tests, and had thereby conclusively ruled out the possibility that the patients had merely fabricated the events. In other words, Freud was an extraordinarily competent expert in distinguishing between fabricated and authentic events. The entire construction of psychoanalytic theory would fall to pieces, and Freud would have had to start everything all over again from scratch, if the seduction events should turn out to be non-authentic. Although Freud did not need any external verification of these events, he had actually obtained such testimony by outsiders, for two of the 18 patients.—Freud suggested that he had discovered a “caput Nili” in neuropathology, an expression which might be translated “a veritable corner-stone”.

§224. Some 10 years later Freud claimed that he had in 1896 been unable to distinguish between authentic and fabricated events. But he had meanwhile learned to do so. Thereby he had discovered that his patients had merely fabricated the events. And after 8 further years he would explain that fabrications of having been sexually seduced were distorted versions of events of infantile masturbation.

Freud wisely abstained from explaining how fabricated events could have been verified by external observers. Or how he could know that his patients had mas-

turbated at the age of 2–4. But he made it clear that the change from the authenticity theory to the fabulation theory, was no more than the substitution of a petty detail which could be made with a stroke of the hand, whereby the entire psychoanalytic construction would remain intact and in need of no further modification. The point was that the seduction fantasies were to the patients “*real in the psychological sense*”. Hence, Freud had made a mistake hardly worthy of mentioning.

This kind of easy substitution of details is a *typical* feature of pseudo-scientific theories. Unsurprisingly, Freud was completely unaware of the fact that the fabulation theory is incompatible with the psychoanalytic core theory. Symptom removal is claimed to be contingent upon *true insight* into the authentic historical circumstances. But now we are told that hysterical symptoms will disappear as soon as the patient starts to *fabulate* about having been sexually seduced. And that it is quite unnecessary that he or she should ever obtain any degree of insight into the fact that these instances of fabulation are defensive derivatives aimed at shielding off awareness of masturbation. Note, Freud never chose the option that the dozens of instances of symptom removal (each patient had more than one symptom) might have been produced by suggestion. But if they had, Freud would have made the clinical experience that suggestive therapy is very much more efficacious than psychoanalytic treatment.

The retraction of the seduction theory is an exceedingly complex phenomenon. Masson (1984) has done much to elucidate the history of the development. But even his account is too simple. Despite the fact that Freud repeatedly retracted the theory in print, he continued in his private practice to construct interpretations that are perfectly analogous with the one given of G.de B.’s oral eczema, cf. §6. Later, I shall present an altogether analogous interpretation delivered in 1922.

It is an illuminating fact that the fabulation theory came to be the official psychoanalytic standpoint at some time between 1906 and 1914, and remained so until the 1980s.

§225. But Alice Miller (1983) and Jeffrey Masson (1984) presented a new variant. They suggested that Freud’s original theory was true, after all. The patients had indeed been seduced, and the seduction events were indeed causally responsible for their symptoms. Moreover, Freud had indeed in the 1890s obtained valid evidence for both these facts. Unfortunately, Freud’s objective scientific contributions produced intense and prolonged hostility. Because of cowardice and against his better knowledge, Freud eventually retracted his true results.

Masson is the first writer who suggested that the 10-year-period of isolation had anything to do with the seduction theory, and that it started in 1896. Freud himself never entertained any such view. The simple truth is that Freud felt isolated during his entire life, however great his success.

Up to the early 1980s the seduction papers were regarded as historical curiosities, which were hardly ever read except by biographers. But suddenly hundreds of thousands of academicians and lay men read the third seduction paper, and numerous persons of all kinds made public comments.

§226. Miller's and Masson's re-interpretation is highly surprising in many respects, some of which are not very conspicuous. I shall list a few ones. Until the most recent years we have incessantly been told that the psychoanalytic method is so powerful, that it will invariably yield infallible information as to what is buried in the depth of the patient's unconscious mind, and also as to what happened during the patient's distant past.

"He that has eyes to see and ears to hear may convince himself that no mortal can keep a secret. If his lips are silent, he chatters with his finger-tips; betrayal oozes out of him at every pore." (GW-V:240/SE-VII:77f.) [Q-226:1]

But if the re-interpretation is correct, then thousands of psychoanalysts have continually observed the same kind of circumstances as Miller and the early Freud. Then why did no other psychoanalyst assert the true state of things many decades earlier?

Some people prefer to falsify history and claim that Freud met much hostility in the 1890s. Just for the sake of the argument, let us accept their claim. But what then about Freud's successors during the 1950s, 1960s, and 1970s—to say nothing about the fact that *the majority* of psychoanalysts reject the re-interpretation even in 1990. Do and did they not dare give public testimony as to the unambiguous nature of their clinical observations, because they rightly fear(ed) to become the target of intense and comprehensive hostility? Who have manifested any hostility against Masson and Miller, apart from conservative psychoanalysts?

§227. If thousands of psychoanalysts unanimously observed authentic instances of infantile seduction: were all of them deliberate liars who gave false testimony in public? Or was the entire psychoanalytic profession distinguished by a degree of self-deception unheard of within any other behavioural school? Were all psychoanalysts hypocrits, when they boasted of conducting a variety of treatment based on unconditional truth?

And *how* did they manage to cure any patient, if they took the utmost care to prevent de-repression of infantile seduction events? Moreover, *how* did they know the nature of the not-yet-de-repressed event? Wouldn't they rather have had to prevent all instances of de-repression?

What did they do if, by some unfortunate accident, they happened to de-repress an infantile seduction event? Would they apply persuasive techniques to produce the false belief that it was just a screen memory?

Suppose they would permit the patient to retain his true recollection. But! *numerous patients will eventually become practicing psychoanalysts*. Will the latter be akin to a person who has the most vivid recollection of having received a bicycle on his 5-year-birthday, but who nevertheless asserts, repeatedly and with considerable ardour, that whenever anyone claims to have received a bicycle before he started school, he is the victim of a sheer fantasy?

§228. So far, I have described three different views as regards Freud's early seduction papers. But nothing more than common sense is needed for seeing that all of them are completely false.

I shall not present a fourth and true interpretative view. Instead, *I shall simply give an account of the non-interpreted content of Freud's paper, such as it really is, and such as it can most easily be seen to be.*

1. Freud makes it absolutely clear that none of the 18 patients claimed to have been sexually seduced. On the contrary, all of them denied having been so. The seduction events were constructed by Freud himself, in exactly the same way as in the case of G.de B. (cf. §6). In fact, Freud explicitly proves the reality of the seduction event from the fact that the patients deny them: they would not have denied that the events really took place, unless they felt ashamed of the events; and it is absolutely impossible that they could have felt ashamed unless the events really took place.

2. Freud admits that *some of the 18 patients were not cured. And he makes this admission in such a strange formulation, that there are strong reason to suppose that none of the patients were cured.*

(Freud's private letters reveal that it is really true that none of the 18 patients recovered. But his readers should only be criticized for not having noticed what is transparent in the printed text.)

3. *It is not true that the sample consisted of 18 patients.* There are reason to suppose that Freud had only four concrete individuals in mind, when he claimed to have 18. And there is some reason to guess that one of these four individuals was neither sick nor a patient of Freud's, but was merely a friend of one of his actual patients.

4. *Only 442 words of the entire third seduction paper are devoted to the description of clinical observations.*

5. *The bulk of the paper consists of distraction maneuvers aiming at concealing the almost total absence of any evidential support.*

6. *The infantile events did not consist of sexual intercourse.*

7. *All the victims were 2–4 years old when the events took place.* In other words, instances of a stepfather forcing a girl of some 10–12 to submit to sexual intercourse, were not mentioned by Freud.

8. *In some cases the only seducer was another young child.*

9. In so far as Freud has been able to find out, SEXUAL SEDUCTION AFTER THE AGE OF 8 CANNOT LEAD TO ANY PSYCHOPATHOLOGICAL REACTION.

When Alice Miller claims that her clinical experiences are in excellent agreement with the ones stated in Freud's third seduction paper, I wonder first and foremost whether they are in agreement with the 9th statement.

Note, *one need not be a textual analyst in order to perceive the presence of at least 7 of the above statements in Freud's paper. Ordinary reading competence will suffice as regards at least all but no. 2 and 5.*

§229. I shall focus upon Jeffrey Masson. He has evidently a much higher IQ than I have. And he has given no sign of possessing any permanent personality trait which could explain his error. Neither could the latter derive from ignorance: Masson has re-translated the entire third seduction paper. It is simply impossible



that he did not detect at least 7 out of the 9 statements listed in the preceding paragraph. Nonetheless, the discrepancy between Freud's text and Masson's misrepresentation is gigantic. We are entitled to entertain the suspicion that some irrational aim may be found behind his innovation. Could any such aim be identified?

I am under no obligation of digging out Masson's secret motive, nor of explaining *the general reception of his paper*. Many people might agree that a book like *The Assault of Truth* would, if it had been published in 1944, 1954, 1964 and perhaps even in 1974, have attracted much less interest.

I shall present a tentative explanatory hypothesis.

§230. Since the 1890s Freud and his followers have been perfectly aware of the fact that their variety of treatment have little therapeutic efficacy, if any at all. They have always been aware of not telling the truth, when they claimed that 100% of the patients will recover, apart from the microscopic fraction of individuals who have an extremely obstinate will to remain ill. But psychoanalysts have also been prepared to sacrifice human health and happiness for the sake of their own prestige and economy. During generations they succeeded in suppressing all effective therapeutic approaches. As long as patients could not turn elsewhere, psychoanalysts bothered little about their own incompetence.

Behaviour therapy existed at least since 1924, and possibly since 1904. But few people—and even few psychologists and psychiatrists—had heard about this approach until Hans-Jürgen Eysenck in 1960 published *Behaviour Therapy and the Neuroses*.

The secret suppression of any knowledge of behaviour therapy, was a most effective strategy until 1960. But an entirely new strategy was needed after that year. Repeatedly during history, psychoanalysts have conducted the most hateful campaigns against concurrents who might threaten their hegemony. This problem will be discussed later. There has been three main targets: Pierre Janet, pharmacology, and behaviour therapy. Most of my readers have been eyewitnesses of the last campaign. The device of a prolonged personal psychoanalysis turned out to have extensive "political" consequences. Mass media people: journalists, fictional writers, movie directors, and so on joined the attempt of depriving individuals in desperate need of help, of effective treatment. The climax may be Anthony Burgess' and Stanley Kubrick's *A Clockwork Orange*, an ethically irresponsible product that is no second to Nazi movies such as *Der ewige Jude* and *Jud Süß*.

§231. A public campaign is much less successful than the secret suppression. Impotent males, individuals vomiting daily during anxiety attacks, and others at the wedge of committing suicide: such people will not be inclined to undergo treatment for 4–6 years even if they are *definitely promised* recovery at the end of the period—if they have the option of recovering after 4–6 weeks. Hence, the campaign against behaviour therapy strongly *decelerated*, but could not *prevent* what Eysenck has termed "the decline of the Freudian empire".

This decline is revealed by objective figures, which I have cited after Rachman & Wilson (1980:52). *At the Columbia University Psychoanalytic Clinic, the*

number of patients applying for psychoanalytic treatment was in 1964 as high as 803. In 1967 the number was reduced to 500 (= 62%). In 1971 the figure had dropped to 162 (=20%). The number of students applying for training in psychoanalysis likewise dropped markedly.

In both respects the well-known phenomenon of a *cultural lag* may be observed in the Scandinavian countries. As late as in the middle 1970s a public decision was made of public mass training of dynamic psychotherapists. Despite their incompetence in curing any patients, these therapists will usually be guaranteed a job at public clinics and hospitals until retirement.

But the development was retarded; it was not stopped. If the just mentioned decision had not been taken during the period when the attacks against behaviour therapy were still predominant and permanent occurrences in academic courses and mass media, it is unlikely that the decision would ever have been taken.

§232. It goes without saying that the rise of behaviour therapy has already had serious consequences for the economy of psychoanalysts in private practice. Some of these show more foresight than others. They realize that some kind of radical change is mandatory if psychoanalysis are to survive. New markets must be found.

Exactly at the critical time world-wide attention was given to the problem of sexual abuse of children. In the U.S.A. a family member convinced of incest is sometimes given the options of going to prison or undergoing psychotherapy. As a camouflaged fine the latter option might have a limited efficacy. As a means of improving the economy of underemployed psychoanalysts, this kind of judicial decisions are much more powerful. Incest clinics are opened all over the U.S.A.

*Masson's and Miller's re-interpretation provides a most impressive guarantee for the unique skill and competence of psychoanalysts in handling such matters.* Allegedly, their great theoretical leader was nearly a century ahead of his age. Freud discovered the reality of sexual abuse of children. He discovered the specific nature of the pathology arising from infantile seduction. He invented an altogether innovative method which was highly effective in undoing the harm caused by forced incest. One could hardly imagine a fabrication that would be more advantageous for the psychoanalytic profession—nor a time at which this particular fabrication would be more profitable.

§233. Strictly speaking, we have two different hypotheses: the first one about Masson's and Miller's personal motives, and the second one about the cause of the enthusiastic reception of their contributions. Both hypotheses are tentative, and I shall not try to present any genuine support.

While there is a limited room for doubt as to what kind of motives Miller and Masson had, there is no room for the hypothesis that they are genuine researchers who happened to make a mistake.

§234. As I have already said, many proponents of psychoanalysis have realized that this theory has little prospect of surviving, unless it undergo some transformation. Some people suggest an admixture of Marxism should be added; others prefer existentialist philosophy or even occultism. But a *recurrent strategic*

*device for camouflaging the issue, is to present the suggestion of the particular remedy as AN ATTACK upon psychoanalysis.*

Perhaps it may be debated exactly what Miller and Masson have done. But their contributions have repeatedly been presented by others as severe critical attacks upon Freud. A Swedish newspaper psychologist even wondered whether Alice Miller's theory will be the little stroke that will fell the psychoanalytic oak.

Any unbiased reader will immediately see that Miller's and Masson's version strongly *increases* Freud's prestige and merits. According to the traditional version, 1. Freud was during 10 years convinced of the absolute truth of ideas for which he had obtained *no* empirical support; 2. he had made a very foolish and flagrantly inexcusable mistake; 3. Freud's critics were much more correct than he himself was, despite the fact that he had studied the relevant phenomena both extensively and intensively; 4. Freud had something to learn from his critics, and he eventually came to perceive the true state of things by accepting the view of his critics; 5. hence, a minimum of common sense turned out to be a much more effective tool, than extensive clinical research.

§235. All these circumstances are reversed by Miller's and Masson's re-interpretation. 1. Freud had obtained conclusive empirical support for his ideas and, hence, had just reason to be convinced of their truth. 2. There was nothing foolish about his ideas. 3. His critics were altogether wrong. 4. Freud had nothing to learn from his critics. 5. If anything, his critics were as lacking of common sense as they were ignorant of the nature of the clinical reality in their own consultation rooms.

In fact, Freud had merely done what Galileo had done previously, that is, he had knowingly submitted to a false theory which was supported by social power.

Moreover, however severely someone might condemn Freud's retraction, the latter conferred no prestige upon his opponents.

§236. Many proponents of the traditional view have praised Freud's open-mindedness and objective scientific attitude, illustrated by the facts that he was prepared to change his view in the light of new evidence, while his early mistake was no barrier to subsequent veracious observation. This idea sounds like "psychoanalytic rationalization". There is much more reason to suspect that a psychiatrist who was capable of maintaining such a degree of gullibility during 10 years, may not have been trustworthy after this period.

§237. It might seem as if the subject of the first two volumes is somewhat heterogeneous. What has the anal theory to do in a report originally aimed at scrutinizing the seduction theory?

*The comparison of the seduction theory and the anal theory will reveal one of the most fundamental features of psychoanalysis. Clinical observations and empirical generalizations are not only fabricated ad hoc, whenever needed to support some theoretical proposition or some interpretation in an individual case. They will likewise disappear in silence and ad hoc, whenever they are no longer "useful".*

Allegedly, Freud was in 1896 in the possession of an abundance of observa-

tions according to which constipation is a genuinely hysterical symptom caused by sexual abuse at the age of 2–4, e.g. finger masturbation in the anus. And Freud had invariably observed that constipation will disappear as soon as the patient recalls the repressed seduction event. This observation was claimed to be in perfect agreement with the theoretical proposition that symptoms are invariably produced and maintained by repression, and will invariably disappear when the authentic causal event has become de-repressed.

This general theoretical proposition was *not* retracted when the authenticity theory was substituted with the fabulation theory. But if Freud told the truth, he was still in the possession of an abundance of observations according to which constipation is a genuinely hysterical symptom, which will disappear as soon as the patient starts to *fabulate* about sexual abuse at the age of 2–4. Since the recounts of seduction events allegedly were attempts at camouflaging acts of masturbation, any genuine scientist would like to know whether the 2–4 year old children had performed *anal* masturbation or, if not, *why* they had camouflaged genital masturbation by means of *this* kind of fabrication.

§238. But in 1908 (in *Charakter und Analerotik*), all Freud's previous clinical observations allegedly proved that constipation is closely related to compulsive neurosis. And this syndrome was thought to be antagonistic to hysteria, apart from mixed cases. And in 1908 Freud had (allegedly) observed over and over again that even the most difficult cases of constipation, which had so far resisted all other therapeutic interventions, would immediately recover, when the patient was told that there is a close causal connection between his constipation and his stinginess. Whatever the reason, Freud does *not* add the qualification that the patient must *accept* this interpretation.

And in 1908 the wealth of previous observations of a discrepant nature quietly disappeared.

It is perfectly legitimate for a researcher to entertain highly discrepant *theories* at various periods of his life. It is an entirely different matter to make incompatible universal *observation statements*, viz. to claim at one time that each and all his previous observations corresponds to the formula "all As are Bs", and to claim at a later time that each and all his previous observations corresponds to the formula "No A is B".

§239. Many examples of the same kind will be supplied throughout the present report, and a few instances have already been given.

In 1905 Freud had allegedly verified in both the case-study of Dora and in "many" previous cases that asthma was caused by spying upon sexual intercourse performed by fathers with sick lungs. Recall from §17 how Freud invented and verified this etiology by means of *the principle of similarity*.

I have not been able to find *the anal theory of asthma* anywhere in Freud's own writings. However, this theory is advanced since the late 1920s by so many of his followers, that the conclusion is inescapable that the theory had Freud's own sanction. Moreover, Fenichel (1945) and others have during the 1940s and 1950s taken for granted that the anal theory of asthma is a non-innovative part of

normal psychoanalytic theory. They have joined Freud in "forgetting" the early spying etiology.

Not unexpectedly, the later theory is likewise based upon *the principle of similarity*. Attacks of asthma may be released by bad air. And a fart is an instance of bad air. This hyper-primitive similarity relation is taken to prove that asthma is a pathological derivative of the sexual pleasure at defecation which any human being is said to have felt during some period of his childhood.

§240. In 1896 each and all 18 patients in a sample of 18 patients were said to have recovered from each and all their symptoms. In 1917, in *Vorlesungen*, Freud asserted an empirical generalization based on all his previous clinical observations: psychoanalytic treatment is free from the defect pertaining to hypnotherapy, viz. that some of the patients will not recover. At the same time, psychoanalytic treatment will provide guarantee for life against relapse into any psychic symptom.

But in 1932, in *Neue Vorlesungen*, Freud presented it as a merit that the rate of recovery after psychoanalytic treatment is lower than the recovery rate after a visit to Lourdes.

§241. In the third seduction paper of 1896 the implication is clear. The active therapeutic agent is the genuine lifting of repression followed by a genuine recollection of that historically authentic event which was in actual fact causally responsible for the genesis and maintenance of the symptom.

In *Charakter und Analerotik* from 1908 Freud had made an abundance of clinical observations to the effect that the mere delivery of an interpretation will produce symptom recovery (cf. §238).

In *Vorlesungen* from 1917 Freud stated that he had at an earlier period entertained *the theoretical conviction* that the mere belief in an interpretation will automatically lead to symptom removal. But he had eventually made the clinical observation that this is not so.

In *Konstruktionen in der Psychoanalyse* from 1937, Freud claimed to have made the clinical experience that: if the psychoanalyst succeeds in producing a firm conviction in the patient, of the truth of an interpretation, this conviction will have *the same therapeutic effect as a genuine instance of lifted repression*.

§242. It would be tempting to list many more instances of the same phenomenon. But I cannot anticipate my entire report in the present chapter.

So far, I have explained the peculiarities of my methodology—or rather, the absence of any peculiarities of the latter. I have explained why I chose the seduction theory as the subject of the present report. I have rebuked the expected objection that the subject of my report is heterogenous. And I have suggested two tentative hypotheses as to the cause and reason behind the present misuse and misrepresentation of Freud's seduction theory.

Schiller said (in collaboration with Goethe) that *the most difficult thing is to see with the eyes what is right under the eyes*. There are a wealth of things which may be established in the most simple and straightforward way, by means of no more than the utmost simple forms of common sense. Strangely, exactly these

things are very often overlooked or forgotten—while complex analytic tools are applied which will necessarily yield altogether invalid results, until the *trivial* or even *hyper-trivial* foundation has been denoted.



## Listing the Totality of Clinical Observations in the Third Seduction Paper

*If Buckingham is nothing but a poor amateur, Gloster is so much more unerring in his art. Fantasy is the poison he injects into his victims. He is playing within their imagination. Like the actors of the Elisabethian scene he transmutes the empty air into anything, and life and meaning are brought into the actor, and the crowd keeps up with things and believes in the Emperor's new clothes.*

Alf Sjöberg

§243. Even the most cursory glance at the third seduction will reveal a most peculiar feature: hardly any clinical data are described in the latter. Amazingly as it may seem, this feature has gone unnoticed for generations.

Disregarding counting errors, the original German text consists of 9047 words. Out of these, as little as 442 (= 4,9%) are devoted to the description of clinical data. And these descriptions are severely lacking in concentration: at least 100 words could easily be deleted.

Moreover, the clinical observations are interspersed throughout the paper in a most confusing way. It is by no means directly perceptible what symptoms are associated with what infantile and non-infantile events.

To impose some structure upon the data, my first step was to give individual names to the patients, in alphabetic order, beginning with the females. I shall now list *the totality of clinical observations stated anywhere in the third seduction paper.*

### MICHAEL

*Symptoms:* one of his legs was stiff. Prior to the emergence of this symptom he had for years paid much attention to the function of his legs.

*Original causal event:* Michael had during early childhood been trained to stimulate an adult female (or several adult females?) by applying his foot in her (their) vagina.

(GW-I:453/SE-III:215)

*There is no further information about anything related to Michael.*

### ALICE

*Symptoms:* Painful sensations in vagina. Violent self-reproach because she had tolerated a boy's stroking her hand affectionately.

*Events antecedent to symptom emergence:* While they were sitting at a table, a boy was stroking her hand affectionately and, at another occasion, pressed his knee against her dress. His facial expression imparted to her the idea that he was doing something forbidden.

*Age at antecedent event:* Puberty; Alice was a young lady.

*Additional comments:* It is explicitly stated that the antecedent events "have been discovered with so much trouble and extracted out of all the mnemonic material". Apparently, the following description is likewise intended to bear upon these antecedent events: the patient did not recount these stories "spontaneously"; the latter were obtained "under the most energetic pressure of the analytic procedure, and against an enormous resistance." The recollection had to be "extracted from [her] piece by piece."

(GW-I:436, 437, 455, 418/SE-III:200f., 217f., 153)

There is no further information about anything related to Alice.

## BEATRICE

*Symptoms:* Anxiety attacks. Painful sensations in vagina.

*Event antecedent to symptom emergence:* A joking question (apparently directed toward someone else) which hinted at an obscene answer.

*Age at antecedent event:* Puberty; Beatrice was a young lady.

*Additional comments:* It is explicitly stated that the antecedent event has "been discovered with so much trouble and extracted out of all the mnemonic material". Apparently, the following description is likewise intended to bear upon this antecedent event: the patient did not recount this story "spontaneously"; the latter was obtained "under the most energetic pressure of the analytic procedure, and against an enormous resistance." The recollection had to be "extracted from [her] piece by piece".

(GW-I:436, 437/SE-III:201)

There is no further information about anything related to Beatrice.

## CHRISTINA

*Intermediary sexual events:* No information as to their nature, but they were performed together with her brother.

*Age at intermediary sexual events:* Later childhood (whatever that may mean); these siblings had also performed acts at an earlier time (whatever that may mean).

*Additional comments:* Christina's brother had not become ill.

(GW-I:442/SE-III:206)

There is no further information about anything related to Christina.

## DESIRÉE

*Symptom:* No information as to its nature, but it was the same as that of Elsa.

*Original causal events:* Group sex ("scènes à trois") involving Elsa and a "male person" (child or adult?); non-specified sexual acts involving the same male person but not Elsa.

*Additional comments:* Because of the perfect agreement between the accounts provided by Desirée and Elsa, Freud had obtained external verification of the events he dugged out by de-repression in the case of Desirée.

(GW-I:442f./SE-III:206f.)

*There is no further information about anything related to Desirée.*

## ELSA

*Symptom:* No information as to its nature, but it was the same as that of Desirée.

*Original causal events:* Group sex ("scènes à trois") involving Desirée and a "male person" (child or adult?); non-specified sexual acts involving the same male person but not Desirée.

*Additional comments:* Freud makes it absolutely clear that the cases of Desirée and Elsa together comprise ONE, not TWO cases of external verification. It never occurred to Freud that: if Elsa has confirmed Desirée's account, then Desirée has reciprocally confirmed Elsa's account.

(GW-I:442f./SE-III:206f.)

*There is no further information about anything related to Elsa.*

## FLORENCE

*Symptoms:* Anxiety attacks; the latter "preferred" certain hours of the day (there is no informations whatever as to what hours were preferred, nor as to how much they were preferred); demand that a certain one among Florence's numerous sisters should be present during the attacks for the purpose of calming her down (it is by no means clear whether this purpose was achieved).

*Original causal events:* An adult male sometimes visited Florence's home. At each visit he asked whether the just mentioned sister was at home. If she was not, he performed a sexual act upon Florence, since he did not fear to be interrupted if this particular sister was absent. There is no information as to the nature of the act.

(GW-I:453/SE-III:215)

*There is no further information about anything related to Florence.*

## THE REMAINING 11 PATIENTS

*There is no information about anything related to them (except their sex).*

§244. (I hope I shall escape the experience of persons who do not read German invoking James Strachey's curious translation in support of the objection that my accounts are incorrect.)

It is immediately seen that all significant information and almost all non-significant information is missing for each and all patients.

An *IR symptom* is a symptom with an infantile root. In the present cases the latter consists of a seduction event. A *non-IR symptom* is a symptom which has not been caused by any infantile event at all.

Let us survey the missing information. We are told the number of patients; but

already at this point there is some inconsistency, the implications of which we shall scrutinize in due course. And we are told their sex.

But for 14 patients out of 18, no symptom at all is described. And for no single patient are we provided with an exhaustive list of all symptoms he or she had, if the general rules stated by Freud are taken serious. Inter alia, Freud claims that *any* hysteric will have both IR symptoms and non-IR symptoms (*pluralis!*) (GW-I:451/SE-III:214). But no non-IR symptom is indicated for any patient; not even the statement can be found in the paper than any particular patient had a non-IR symptom.

§245. We are told something about what a non-IR symptom is *not*. All the following varieties are invariably IR symptoms:

“the numerous and diverse sensations and paraesthesias of the genital organs and other parts of the body, [...] painful need to urinate, the sensation accompanying defaecation, intestinal disturbances, choking and vomiting, indigestion and disgust at food.” (GW-I:451/SE-III:214) [Q-245:1]

The reason is obvious, and is almost explicitly stated. These symptoms are SIMILAR to sexual intercourse, coitus per rectum, fellatio etc. And on the basis of the prejudiced idea that *the cause is similar to the effect*, Freud deduces the idea that such symptoms are invariably caused by such sexual experiences during infancy.

But we are not provided with any instance of a non-IR symptom. However much we may read the third seduction paper or Freud's *Gesammelte Werke*, we shall not learn whether IR and non-IR symptoms are invariably structurally dissimilar, or whether some symptoms might have a different character in different patients.

For only two patients is there any indication of their age at the onset of an IR symptom. For no patient is there any information as to his or her age at the start of the treatment; nor as to the rather important question of the duration of the interval between the onset of the symptom and the start of the treatment.

Nor are we told anything about the duration of the treatment, whether measured in number of months or in number of consultations.

§246. NOWHERE IN THE THIRD SEDUCTION PAPER CAN ANY STATEMENT BE FOUND TO THE EFFECT THAT a. ALL 18 PATIENTS WERE CURED; b. ANY PARTICULAR PATIENT WAS CURED; 3. AT LEAST ONE PATIENT WAS CURED.

It is a disheartening fact that thousands of readers imagine that they have read all three statements in the paper. But the only thing that *is* found in this respect, is a *persuasive insinuation* of recovery. The latter will be analyzed in the following chapter. If we interpret this device as a literal statement, we are told nothing about the duration of the interval between the start of the treatment and the disappearance of the symptom(s), whether measured in number of months or in number of consultations.

For only two patients is any event described which preceded the emergence of

an IR symptom. And these descriptions are singularly lacking as to details. Moreover, we are not even informed as to whether non-IR symptoms must or might be preceded by antecedent (releasing) events.

For only one patient are we supplied with some vacuous information as to a set of intermediary events, together with a curiously indeterminate indication of the patient's age at the time. It should be noted that these intermediary events are altogether irrelevant; they have no logical place at all in the paper.

§247. *Evidently, the nature of the de-repressed original causal event constitutes one of the most crucial pieces of information. But for only four patients are we provided with any indication at all as to any such event. For one of these, nothing but a few marginal and rather trivial aspects of the event are mentioned. For the remaining three the information is markedly underspecified.*

*For no single patient are we supplied with an exhaustive list of all de-repressed seduction events.*

Even more surprising is the fact that INFORMATION ABOUT BOTH THE SYMPTOM(S) AND THE DE-REPRESSED ORIGINAL CAUSAL EVENT(S) IS SUPPLIED FOR ONLY 1 (=ONE) PATIENT. And even this information is deficient as to both symptom and event.

It is possible that Desirée and Elsa are identical with Alice and Beatrice. Without some detective work the reader would have no chance of discovering this circumstance. If they are really identical, we are provided with information on both symptoms and fragments of the causal events for two further patients. But at the same time the number of patients for whom we are given no information at all, will raise to 13.

Many *generalizations* in the paper are flagrantly contradicted by the biographic data or by other generalizations. Consequently, we are not entitled to include into the biographies a generalization stated elsewhere, viz. that all patients were 2–4 years old at the time of the seduction events.

§248. I shall now turn to the utmost important point. *Freud's theory requires an exceedingly brief interval between the occurrence of de-repression of the original causal event, and the disappearance of the symptom.* It would be natural to expect an interval of at most a few minutes. But I shall generously accept a maximum of 24 hours. However:

FREUD IS COMPLETELY SILENT AS TO THE DURATION OF THE INTERVAL BETWEEN RECALL AND SYMPTOM DISAPPEARANCE.

A few final points. We are told nothing about the duration of the follow-up period, nor about the temporal interval during which no relapse occurred.

§249. I shall now display most of this information in Table 249:1. Cells devoid of any information are dashed. Among the 378 cells of the table, no less than 326 are dashed. If we start out from the patients, no information of any kind is supplied for 11—or possibly 13—of the patients. If we start out from the 21 information categories, no less than 14 of these are dashed for all patients.

Turning to the 52 cells that are not dashed, 18 indicate the sex. The remaining 34 cells are altogether wanting as to concrete details.

Complete absence of any information is indicated by

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How could such a remarkable pattern be explained?

§250. In §§376ff. I have constructed a biography of a fictive patient, Victor. If any such biography be ever observed in the empirical world, Freud's seduction theory will indeed be proved to be true of at least one individual. I have already noted that the seduction theory could not be proved by any configuration of data widely at variance with the one I have constructed.

An inescapable consequence it this. If Freud told the truth, he was in the possession of 18 biographies of exactly this variety.

In a paper of 9047 words there would be room for 9 such biographies. And I shall anticipate a result that will eventually be conclusively supported. *If Freud had presented even one single such biography, nearly 100% of the content actually found in his paper would have been absolutely superfluous.* For instance, he would not have had to advance curious sophisms to "prove" that there is nothing inherently implausible in the idea that some children have indeed been victims of sexual abuse.

§251. At the present place I shall focus on a different aspect. I shall ask the reader to read the biography of Victor now (cf. §§376ff.), because I would like him to perform a thought experiment. Let it be assumed that the reader 1. has obtained 18 patient biographies of the same nature as the just mentioned one; 2. has obtained them during the preceeding 171 calendar days; 3. is about to write a paper to be orally read to an association of prominent psychiatrists, a paper which will later be printed in a scientific periodical.

The reader's task will be to produce a paper containing as scarce clinical information as the third seduction paper. The biography of Victor must be reduced to the biography of Michael. The remaining biographies must either be altogether deleted, or reduced to the kind of biographies cited in §243.

I do not ask the reader actually to produce such a paper, but to reflect upon the problem how he would proceed in order to solve the task.—The latter is not very difficult. The reader may just make the deliberate decision to delete all non-trivial and almost all trivial information from any given biography.

§252. But now I shall add a further condition. The reader must arrive at the just described *end product*, but he must not make any *decision* of deleting any *particular piece* of information from the final paper.

Let us compare this task with another. The reader is about to produce a paper. He is requested to make the general decision to include a very high proportion of spelling errors in the paper. At the same time he is forbidden to make any particular decision to include any particular spelling error (e.g., the 7th word on the 19th line shall be spelled "hwo").

A variety of techniques are available. Before writing, the reader may produce tiredness by taking sedatives, depriving himself of sleep, working for protracted hours etc. Concommittant with writing, the reader may produce distraction, by performing a second task needing close attention. In addition, he may copy the final paper a thousand times and use only the last copy.

In other words, the typical kinds of errors associated with *carelessness* can in-

deed be produced by a will act, although none of the particular instances of these errors are produced by any particular will act.

§253. Could any of these techniques be applied to the former task? Obviously, one would have to be not merely tired nor extremely tired, but near unconscious, before one would be capable of reducing the biography of Victor to the biography of Michael, without making particular decisions.

And it is seen at a glance that Freud was by no means nearly unconscious when he wrote the third seduction paper. In the following chapter we shall see that some of his formulations reveal that he was actually in a state of *increased* attention.

Consequently, it is a sheer impossibility that the reduction of a biography of the same nature as that of Victor, to a biography of the same nature as that of Michael, could have derived from carelessness.

§254. On the other hand, it is likewise impossible that the reduction could have derived from a deliberate decision. No non-surrealistic motive could be suggested, which could have induced Freud to delete all non-trivial and nearly all trivial observations from the published version.

The only possible explanation is therefore that no reduction took place. This statement entails the following one: Freud had no secret comprehensive and non-trivial biographies to reduce. If a body of authentic observations of the nature postulated by Freud had been present in his mind, no casual factor which had any chance of really being present, could have amputated each and all non-trivial details.

## Were the Patients Cured?

*The immediate conclusion I came to after 10 minutes of reading was that Burt was a fraud.*

Leon Kamin

*A few minutes' scrutiny by a skeptical observer some two decades later sufficed to show glaring implausibilities in Burt's statistics.*

William Broad & Nicholas Wade

*The untruths of both Freud and Burt are so clumsy that half an hour is sufficient to expose them.*

Max Scharnberg

*In other words, no scientific investigation is needed. Common sense, and reading for half an hour, are enough. These are not the words of a serious-minded researcher.*

Lars Gunnar Lundh

§255. The problem to which I shall now turn, is the utmost crucial and central in the entire third seduction paper. Consequently, the present chapter is the utmost important one throughout the first half dozen volumes—with the possible exception of the first two methodological chapters of the first book.

All previous commentators have construed the third seduction paper as containing the definite claim that all 18 patients of the sample were cured. However, *nowhere in the paper is any assertion stated to the effect that all patients were cured. Not even the assertion can be found that at least one patient recovered.*

Evidently, this is a most remarkable fact. But none of the impressive number of commentators during the last decade as well as during the entire century have perceived this most conspicuous feature of Freud's text.

Perhaps even more surprising is the configuration of claims actually advanced by the seduction paper:

I. *For each and all 18 patients of the sample, Freud conclusively verified the following proposition. Any symptom will necessarily disappear, when the infantile causal event has been consciously recalled after lifting of the repression (i.e., after de-repression).* (GW-I:448/SE-III:211f.)

II. *The infantile causal event was consciously recalled after de-repression for each and all symptoms of each and all 18 patients.* (GW-I:435/SE-III:199)

III. *Some of the 18 patients did not lose their symptoms.* (GW-I:435/SE-III:199)

[Q-255:1]

§256. Evidently, that *the negation of the third proposition of Q-255:1 is a sine qua non for the seduction theory.* It simply cannot be true that hysterical symptoms are caused and maintained by repression of events of sexual seduction, and will disappear as such as the repression is lifted—while at the same time some of the 18 patients experienced complete de-repression, but nonetheless retained their symptom. Flagrantly, if the third proposition asserted by Freud and listed in Q-255:1 is true, THEN FREUD KNEW THAT HIS SEDUCTION THEORY WAS FALSE WHEN HE ON 21 APRIL 1896, IN FRONT OF *VEREIN FÜR PSYCHIATRIE UND NEUROLOGIE IN WIEN*, CLAIMED TO HAVE ABUNDANTLY AND CONCLUSIVELY VERIFIED THE SEDUCTION THEORY.

Because of the prevalent atmosphere I have no choice but to give a much more detailed presentation than would be called for on the basis of purely logical considerations.

§257. Did Freud actually assert the three propositions listed in Q-255:1? To this date, no commentator has doubted the fact that the first two propositions are indeed stated in the third seduction paper. Nonetheless, I shall have to prevent future commentators from trying to save Freud by means of such denial. The first two propositions are unambiguously supported by the following quotations:

“Wir heilen sie [= unsere Kranken] aber von ihrer Hysterie, indem wir ihnen die unbewussten Erinnerungen der Infantilszenen in bewusste verwandeln. [...] Diese Szenen müssen als *unbewusste Erinnerungen* vorhanden sein; nur solange und insofern sie unbewusst sind, können sie hysterische Symptome erzeugen und unterhalten.” (GW-I:448) [Q-257:1]

“We cure them [= our patients] of their hysteria by transforming their unconscious memories of the infantile scenes into conscious ones. [...] The scenes must be present as *unconscious memories*; only as long as, and in so far as, they are unconscious are they able to create and maintain hysterical symptoms.” (SE-III:211) [Q-257:2]

“Erst die mühseligsten Detailuntersuchungen haben mich, und zwar langsam genug, zu der Meinung bekehrt, die ich heute vertrete. [...] ich in etwa achtzehn Fällen von Hysterie diesen Zusammenhang für jedes einzelne Symptom erkennen [...] konnte.” (GW-I:435) [Q-257:3]

“Only the most laborous and detailed investigations have converted me, and that slowly enough, to the view I hold to-day. [...] in some eighteen cases of hysteria I have been able to discover this connection in every single symptom.” (SE-III:199) [Q-257:4]

§258. By contrast, the third proposition of Q-255:1 is not found in the third seduction paper in any straightforward way. I shall quote the text of both GW and SE. In addition I shall, for the benefit of readers who do not read German, give an un-English word-by-word translation, in which I have preserved the peculiarities of German syntax as far as possible. This version may be crucial when we turn to the question whether Freud's error was intentionally constructed.

“... ich in etwa achtzehn Fällen von Hysterie diesen Zusammenhang für jedes einzelne Symptom erkennen und, wo es die Verhältnisse gestatteten, durch den therapeutischen Erfolg bekräften konnte.” (GW-I:435) [Q-258:1]

“...in some eighteen cases of hysteria, I have been able to discover this connection in every single symptom, and, where the circumstances allowed, to confirm it by therapeutic success.” (SE-III:199) [Q-258:2]

“...I in some 18 cases of hysteria this connection in every single symptom discern and where that the circumstances permitted by the therapeutic success confirm could.” (GW-I:435/SE-III:199) [Q-258:3]

§259. I shall now spell out in gradual steps the semantic content of the subclause of 4–5 words found in the last three quotations (but deleted from Q-257:3 and Q-257:4.

A. “*The theory could be confirmed by therapeutic success where the circumstances permitted.*”

This statement means that:

B. “*The circumstances did not invariably permit the theory to be confirmed by therapeutic success.*”

The second statement means, in turn that:

C. “*The theory was not confirmed by therapeutic success for all 18 patients.*”

The third statement means, in turn, that:

D. “*All 18 patients did not experience therapeutic success.*”

Or, in plain words:

E. “*Not all 18 patients recovered.*”

The preceding statement may be given an alternative formulation:

F. “*Some of the 18 patients did not recover.*”

[Q-259:1]

§260. I shall in passing mention a few strategic pseudo-arguments which may be used with the aim of saving Freud’s claims. But unless the refutation takes only a few words, I shall waste no space on such refutation. First, it is a “well-known” fact that psychoanalysts by “recovery” means much more than symptom removal. Consequently, all 18 patients might well have lost all their symptoms, although only *some* of them had experienced full recovery involving all the psychodynamic surplus qualities.—But still in 1917 the sole goals of the treatment was symptom removal and guarantee against relapse.

Second, the expression “*some 18 cases*” might mean, say, 20 cases. Consequently, it is possible that all 18 but not all 20 patients had recovered.—But Freud explicitly states that the 18 patients constitute the total sample, and that the next future case he would encounter would be the 19th one.

Third, the occurrence of therapeutic success for a certain patient, and confirmation of the theory by therapeutic success for a certain patient, are two different things.

§261. The specific techniques and criteria for deciding whether a specific er-

ror at hand derives from intention or carelessness, will not be discussed until later, apart from one such criterion described in ch.9. But no more than common sense is needed for seeing that the contradiction in the juxtaposition of Q-255:1 could not possibly have been caused by carelessness. This factor could be responsible for only two varieties of errors, viz. random errors and systematic errors associated with decreased attention. Evidently, the formulations of Q-258:1, Q-258:2 and Q-258:3 could only have been carefully worked out under a high level of attention, except for a mind who had by prolonged use become habituated.

§262. Another result belonging to a later volume must likewise be mentioned and applied to Q-255:1. The third statement illustrates no less than three persuasive techniques.

First, *the crucial information is stated in a subordinate clause. And it is seen that German syntax makes this device particularly effective.*

Second, *the information is delivered as a passing remark when Freud is talking about something else: obviously a semantic construction which will reduce the likelihood that the message is perceived.*

Third, *the message itself is stated in so awkward and unnatural words that it is barely noticeable.*

Any genuine researcher would have chosen a natural formulation, e.g., "some of the patients were not cured". The transformation of this message into the verbal sequence "where the circumstances allowed" clearly reveals the virtuoso.

§263. Flagrantly, at least one of the three statements of Q-255:1 must be false. Suppose that I and II are true, and that Freud indeed succeeded in curing all 18 patients of all their symptoms. In this situation, no believable motive could be suggested as to why Freud should advance the *false* claim that some of the patients did not recover. This is so much the more true, as the entire third seduction paper evidently is aimed at presenting a new and superior therapeutic method. Still more, if the third statement is false, why should Freud both *present* this message and take himself so much trouble of *concealing* the presence of the latter in the paper?

By contrast, once Freud had committed his prestige as a scientist and his economy as a practitioner to the claims that he had invented a specific therapeutic approach which would produce symptom removal by means of de-repression, no specific explanation is needed of the fact that Freud repeatedly and in public asserted that he had indeed produced many instances of de-repression and recovery, even if these assertions had no foundation in his actual clinical experience. Nor is it difficult to understand why he should publish many *faked* instances of recovery following de-repression.

§264. Freud claimed to have experienced "countless successes" with his method. His close friend Wilhelm Fliess did exactly the same thing. Allegedly, the latter had repeatedly observed that massive uterine bleeding would recover, "the very moment" the left middle turbinate bone in the nose was removed. (Masson, 1984:77). It is difficult to communicate with people who reject Fliess'



claim with a stroke of the hand, while they request any researcher to accept Freud's claim at face value.

§265. One objection merits an answer: if Freud had deliberately aimed at deceiving the reader, he would hardly have included the true information in his paper.

First, Freud did not run any great risk, since during some 95 years no one seems to have detected the hidden reservation (or, as I call the phenomenon in §344, the Brask-note).

Second, although I am under no obligation of inferring what motive Freud might have had, one motive easily suggests itself. If, say, Michael and Florence were not cured, there was a non-neglectible risk that they might see some other doctor at a later occasion. And the latter might recognize them as belonging to Freud's sample of 1896: that is, to a sample of patients all of whom were allegedly cured. Freud has prepared for this situation by building a "fire-escape" into his text: he can prove that *under the seal*, as it were, he has clearly stated that some patients did not recover.

It is a most successful technique to include both the true statement and the incompatible false statement in the same paper. If the truth is stated *somewhere* in a paper, numerous academicians are prepared to forgive the writer the presence of an incompatible false statement, even if each and all further conclusions are based on the false statement.

§266. Are we entitled to conclude that *none* of the 18 patients were cured? I shall first reason around Freud's published text, and then I shall review Freud's private correspondence.

Even today, and even if the most superior behaviour therapeutic techniques are applied, hysterics have a poor prognosis. In the 1890s, their prognosis was obviously much less fortunate. As we have seen, 171 calendar days before Freud delivered his report, he had met only two of the patients of the sample. Now, to have successfully cured, say, two hysterics in at most 171 calendar days, by means of an innovative therapeutic approach involving de-repression, would have been a rather sensational achievement. If Freud was able to show up just two patients to some authority (e.g. Krafft-Ebing), his career would be significantly furthered. It would indeed be very foolish to ruin this opportunity, by fabricating additional patients who, in actual fact, had neither recovered nor experienced de-repression.

I feel myself unable to imagine that Freud was not perfectly aware of these circumstances. But since the methodology of textual analysis has not yet been given a systematic presentation, and not even an extensive public presentation, systematic or not, caution is perhaps called for as regards this and the following derivation.

§267. We have already seen that it is Freud's deliberate fabrication that Dora ever recounted the spying event. The number of descriptions of instances of de-repression in *Gesammelte Werke* is exceedingly small. And I shall explicitly prove that an impressive number of these are deliberately faked.

The evidence is conclusive that neither Alice, Beatrice, Florence nor Michael recounted the infantile seduction events put into their mouths. Freud never observed any instance of de-repression.

Now, if Freud had actually cured two patients of their symptoms, he would have made an achievement, regardless of how he had produced this result. And whatever set of facts a patient might recount, it would always be possible to construct a more or less interesting biography.

But the moment Freud added the false claim that de-repression had occurred, as well as the description of the nature of the de-repressed event, and the second false claim that symptom recovery was closely associated with de-repression; at that moment he deprived himself of the possibility of showing up *any* of his patients to *anyone* except true believers. Once more, I feel myself unable to believe that Freud was unaware of this fact. His entire career gives the impression that he was a very skilled strategist. I do not think he would have ruined his own chances of showing up *any single one* out of the 18 patients, if there had been symptom removal to show up about any single patient.

§268. We should not conclude that a writer known to have deliberately fabricated his data at one occasion, has done the same thing at every possible occasion. However, if a writer is known to have *recurrently* and *habitually* fabricated in the most *unrestricted* way, there is reason to be extremely skeptical about *any* claim of unknown truth value he might have advanced.

*The heredity juxtaposition* (Q-15:1) is flagrantly the product of a fabulator. Likewise, it is a sheer impossibility that Freud could have been in good faith, when he claimed that most of his 18 patients had had a minimum of 100 hours of treatment (cf. Q-14:1).

The reader is referred to later volumes concerning two criteria: that of *simple isomorphy* and that of *lateral isomorphy*. But cf. also §§299 and 448.

§269. Let us now invoke the testimony of the witness Sigmund Freud in his private letters.

Recall that Freud on 21 April 1896 claimed 1. to have brought 18 psychoanalyses to a real conclusion; 2. to have achieved complete success; 3. to have observed unconscious memories of childhood experiences breaking through and overcoming the resistance of the conscious.

As I shall show in §480f. Freud claimed to have observed for *each* patient, not 2–4 instances of de-repressed events, but 2–4 *categories* of de-repressed events, where each category may well have comprised several instances for the same patient. It is hardly possible to interpret Freud's text in such a way that the total number of de-repressed events for all 18 patients will not reach at least 60 events.

Now listen to Freud's testimony 17 months later, in his letter of 21 September 1897. He explains to Fliess why he no longer believes in the seduction theory:

"The continual disappointment in my efforts to bring any [psycho-]analysis to a real conclusion;  
[...] the absence of the complete successes on which I had counted.

[...] in the most deep-reaching psychosis the unconscious memory does not break through, so that the secret of the childhood experiences is not disclosed even in the most confused delirium. [...] the unconscious never overcomes the resistance of the conscious." (Freud, 1985:264f.) (Q-269:1)

§270. Cioffi (1986:230) comments upon the very same letter: "We are entitled to suspect that he [= Freud] might not have found the state of these patients so unsatisfactory had he not already decided that the assumptions on which he had treated them were mistaken." Cioffi's comment is quite besides the mark. Freud's *sole* goals in 1896 were 1. symptom removal, and 2. guarantee against relapse. In April 1896 he entertained no more than a vague suspicion that the second goal could be achieved at all. And it would be an inadequate interpretation of his text that he had abandoned the seduction theory because he had observed a high rate of relapse. Moreover, 18 cases of relapse during 17 months in a sample of 18 patients, would certainly be a unique result in the entire history of psychiatry.

Nor can Freud's honesty be saved by means of the fabulation theory. On the one hand, in September 1897 he had not yet invented the latter. On the other hand, the evidence is water-proof that Freud himself invented the seduction events of all 18 patients, just like he invented the spying event and the explanation of G.de B.'s oral eczema (cf. §6).

§271. Let us study further private testimonies. Eight and a half months after 21 April 1896, on 3 January 1897, Freud wrote to Fliess:

"If we [= Freud and Fliess] do not prematurely capsize, if our constitutions can stand it, we shall arrive. *Nous y arriverons*. Give me another ten years, and I shall finish the neuroses and the new psychology. You will probably need less time for your organology.

[...] At our next congress [= personal meeting between Freud and Fliess] I hope there will be important things to talk about. I think BY EASTER at the last, maybe in Prague. PERHAPS BY THEN I SHALL HAVE CARRIED ONE CASE TO COMPLETION." (Freud, 1985:219f., capitals added) [Q-271:1]

The same man who claimed in front of *Verein für Psychiatrie und Neurologie in Wien* to have carried 18 cases to completion, now expresses the vague hope that he *might possibly* succeed in curing his first patient in the near future—about one year after the delivery of his report: Easter Sunday 1897 fell on 18 April.

§272. One of the most noteworthy features of Freud's writings is the absence of progress. Nearly identical formulations are repeated after decades. (Many illustrative examples will be given elsewhere.) One possible explanation is that Freud's formulations were primarily determined by the advantages of the momentaneous situation.

The reader should compare the content of the present paragraph with that of the preceding one.

Let us take a look at a letter Freud wrote on 16 April 1896, that is, *5 days before he delivered his report*. Recall that Freud had already on 5 February 1896 posted two papers on the seduction theory, and had at that time claimed to have cured 13 cases of hysteria (GW-I:380/SE-III:163); and 96 calendar days earlier

he had not even met 11 of these patients. Despite the fact that Freud had (if he told the truth) had just achieved such an impressive result, and despite the fact that he was just about to publish an even more impressive result, he was completely silent about both results. Instead, he talked of what he would probably achieve in the future:

"If both of us are still granted a few more years for quiet work, we shall certainly leave behind something that can justify our existence. Knowing this, I feel strong in the face of all daily cares and worries.

[...] I am convinced that, given certain conditions in regard to the person and the case, I can definitely cure hysteria and obsessional neurosis." (Freud, 1985:180) [Q-272:1]

If Freud told the truth, he did definitely not need a few more years. He had already left behind something that could justify his existence. And he had every reason to feel strong on the basis of his *present* achievements, rather than to console himself by any mere *hope* of what he might do *in the future*.

It is curious hear a man who allegedly had just produced the most impressive therapeutic result related to hysterics in the entire history of psychiatry, express no more than a subjective conviction that he will be able to cure certain selected cases of hysteria.

§273. To sum up, the evidence is clear-cut. *None of the 18 patients were cured. Freud had deliberately fabricated the instances of recovery.*

Freud deliberately fabricated the instances of symptom removal. He deliberately fabricated the events of de-repression. He deliberately fabricated the "fact" that the patients had recounted any seduction events.

Elsewhere I shall suggest that the majority of the very patients themselves are likewise fabricated.

## External Verification of the De-repressed Seduction Events.

*In the natural sciences, men such as Kepler, Newton, Lavoisier, Darwin and Freud have transformed not only our beliefs, but also our ways of arguing and our standards of relevance and proof: they have accordingly enriched the logic as well as the content of natural science.*

Stephen Edelston Toulmin

*Und da zeigt er den Schülern wie sein Perpetuum mobile geht, und schwitzt sehr dabei, und die Schüler staunen das an und werden sehr dumm davon.*

Bettina von Arnim

§274. Among all Freud's proofs of the seduction theory, one may appear particularly persuasive: outsiders who were eye-witnesses of the seduction events have confirmed the authenticity of the infantile events Freud digged out. Since these events were absolutely inaccessible to the patients at the start of the treatment, the evidential power of such testimony is claimed to be very strong.

I shall quote the entire relevant text.

"There would be one other proof, and a really unassailable one, of the genuineness of childhood sexual experiences—namely, if the statements of someone who is being analysed were to be confirmed by someone else, whether under treatment or not. These two people will have had to have taken part in the same experience in their childhood—perhaps to have stood in some sexual relationship to each other. Such relations between children are, as you will hear in a moment, by no means rare. Moreover, it quite often happens that both of those concerned subsequently fall ill of neuroses; yet I regard it as a fortunate accident that, out of eighteen cases, I have been able to obtain an objective confirmation of this sort in two. In one instance, it was the brother (who had remained well) who of his own accord confirmed—not, it is true, his earliest sexual experiences with his sister (who was the patient)—but at least scenes of that kind from later childhood, and the fact that there had been sexual relations dating further back. In the other instance, it happened that two women whom I was treating had as children had sexual relations with the same man ["male person" according to GW], in the course of which certain scenes had taken place *à trois*. A particular symptom, which was derived from these childhood events, had developed in both women, as evidence of what they had experienced in common." (GW-I:442f./SE-III:206) [Q-274:1]

§275. I reject the suggestion that 2 hits out of 18 constitute rather weak support. If Freud told the truth, his "postdictions" were highly remarkable in all cases;

they were more or less different in each case; and he himself had no control as to what cases would be subjected to testing by external testimony.

Nonetheless, the number 2 will eventually turn out to be extremely important because of an unsuspected reason. Cf. ch. 21.

§276. There exists no standard procedure for evaluating external testimony. And numerous highly competent academicians may, after much reflexion, arrive at thoughtless decisions.

Again and again we shall hear that this or that testimony merits credence, because it was made by a trustworthy person. But *the criterion of trustworthiness is neither sufficient nor even necessary*. Moreover, the degree of trustworthiness of a person can hardly be adequately assessed. Quite a few cases are known in which persons eventually ended up in jail because of foolproof evidence of their crime. But a few years previously, several re-known experts (judicial, psychiatric, or others), to whom the fool-proof evidence was available, had guaranteed the absolute trustworthiness of these criminals. A criterion recurrently applied is simply that the assessed individual held a prominent social position:

"An excellent and perfectly honorable man, father of a family, justly honored and absolutely incapable (I will gladly act as a guarantor) of any ignominious action, [...]" (Alfred Fournier: *Simulation d'attentats vénériens sur de jeunes enfants*, quoted in Masson, 1984:43) [Q-276:1]

The criterion of trustworthiness should be abandoned altogether.

§277. Strangely, numerous psychologists neglect the most crucial *criterion of independence*.

Assume the following sequence of events. 1. When the patient was two years old, her uncle was caught in flagrante delictu while practicing fellatio upon her. 2. The patient did not retain any recollection of the event. 3. When the patient was 9 years old, mass media paid close attention to the phenomenon of infantile seduction. Inspired by newspaper reading and television watching, the patient's parents frequently discussed the prior assault upon their own daughter. And they were not aware of the fact that the patient repeatedly overheard their conversations. 4. The patient eventually forgot these secondary events. 5. At the age of 22 she started psychotherapy. After having been treated for a year, the "recollection" emerged of her uncle practicing fellatio upon her while she was still an infant. 6. What had emerged was in actual fact a false memory based upon the parents conversations during her later childhood. 7. The psychotherapist asked the parents about the event. They confirmed that it had really taken place. 8. The therapist concluded that he had succeeded in digging out an authentic recollection which the patient had retained since infancy.

§278. Elsewhere I shall discuss the phenomenon that the very act of questioning an individual may induce him or her to produce sheer fantasies, as well as to experience these as authentic memories.

Here, I shall instead substitute the just presented sequence of events with an essentially different, but rather parallel one. I shall assume that the uncle was in-



nocent, and that the sexual act had unjustly been attributed to him. But the patient overheard exactly the same conversations during later childhood.

Many behavioural scientists are prepared to accept the final conclusion (item 8), if only item 5 and 7 are satisfied. These items are common to both versions.

§279. While *independency* is a *conditio sine qua non*, I shall show why we may dispense altogether with trustworthiness. I shall construct a third version. a. The uncle was a psychotic who would frequently voice the most curious fantasies as to what he had done or experienced. b. He really performed fellatio upon his niece at one occasion when she was two years old. c. On the same day he described the act in some kind of a diary, which he carefully sealed. d. Still on the same day he died from a traffic accident, without having told anyone about the act. e. The seal remained unbroken until his niece was 30 years old. f. At the age of 22 his niece started psychotherapy. After about a year of treatment, she recounted what she claimed to experience as an authentic recollection: her uncle practicing fellatio upon her while she had not yet learned to talk.

All items except item b may be taken as premises. Let the conclusion comprise both item b and the statement that the psychotherapist had digged out a genuine recollection which had been retained in the patient's mind since infancy.

In this situation, I think the conclusion would be strongly supported by the premises. The probability of a pure coincidence between two *independent* fantasies, or between one fantasy and one *independent* authentic account, is microscopic. Numerous behavioural hypotheses have been generally accepted on the basis of much weaker evidence.

If *two* such cases were observed in which the therapist had "postdicted" different infantile seduction acts, I do not see how the conclusion could possibly be doubted.

§280. If we turn to the biography of Christina presented in §243 and, in Freud's original formulation, in Q-274:1, we shall as usual notice Freud's lack of methodological sophistication. If there is to be any sense in Freud's account, the latter must be construed as implying that Christina's brother verified events which Freud had digged out by means of psychoanalytic technique, and of which Christina was absolutely unaware at the start of her treatment.

But if her brother had always had a perfectly conscious recollection of the events, how could we be sure that he had never told his sister anything at a much later occasion? In fact, if his sister was ill, and he knew that she was treated by a therapist who devoted immense attention to sexual matters, we should expect him to try to give a hand: remind her of something she must tell her therapist. This is especially true, if Freud had questioned him.

We should not expect Freud to warn him of telling such things to Christina. Recall from Q-6:1 that Freud was *surprised* when G.de B. told her father about Freud's interpretations. Freud did not even learn his lesson. It never occurred to him, that Dora would most probably inform her father and Mr. K. of the fact that Freud had accepted *her* version around the seduction attempt. He was perfectly willing to accept her as a patient in October 1900, despite the fact that she sub-

mitted to the treatment solely because she was unable to resist her father's command. But in April 1902, when she manifested a genuine will to have treatment because of her extremely painful symptoms, Freud refused. He did so *because* she had not kept his interpretations secret from her father and Mr. K. The same man who talked so unrestricted about sexuality, requested Dora to *guess on her own accord* that no one should be told about his interpretations.

These circumstances will be discussed in a later volume. Here, I simply want to emphasize the fact that Freud would probably not have thought of asking Christina's brother to keep silent. And even if he had, we cannot be sure that the brother would have complied.

§281. The same kind of methodological flaw is revealed by the cases of Desirée and Elsa. In a somewhat different context, which nonetheless has a direct bearing upon the present problem, Freud rhetorically talks of the only alternative to the hypothesis that the seduction events really took place:

"[...] would otherwise lead us to believe that there were secret understanding between the various patients." (GW-I:441/SE-III:205) [Q-281:1]

But Desirée and Elsa might have been cousins, neighbours, former schoolmates, or other kinds of acquaintances. If Desirée finished her treatment before Elsa started hers, Desirée could have told Elsa what she had learned, and it would still have been true that Elsa had at the start of the treatment no *recollection* of the event.

§282. But Freud completely overlooked another and much more crucial circumstance. IF DESIREE'S ACCOUNT HAD BEEN VERIFIED BY ELSA'S CONCORDANT ACCOUNT, THEN ELSA'S ACCOUNT HAD LIKEWISE BEEN VERIFIED BY DESIREE'S CONCORDANT ACCOUNT. CONSEQUENTLY, FREUD MUST HAVE OBTAINED EXTERNAL VERIFICATION FOR TWO PATIENTS BESIDES CHRISTINA, NOT JUST FOR ONE.

We need not bother as to whether carelessness could possibly have produced this kind of distortion *if* 1. Freud had merely had a brief contact with Desirée and Elsa 2. years ago, 3. had given no thought to them during the intervening interval, and 4. had only been given brief presentations of the events. If Freud told the truth, 1. Desirée and Elsa had been intensively treated, most probably for 100 hours or more, 2. during the immediately preceding 171 calendar days. And 3. they had given very detailed accounts of the events comprising hundred of features. Likewise 4. they had presented Freud with thousands of details relating these events to their general life, so that Freud could conclusively rule out the suggestion that they had fabricated the events.

It is a sheer impossibility that Freud could have made such a mistake as the just described one, unless some strange circumstances were involved. The first idea to suggest itself is that Freud had no real individuals in mind, with real names, real hair colour, real nose forms etc. In fact, one of the standard features of a fabricated account and, consequently, one of the criteria for identifying accounts deriving from fabrication, is related to the narrator's lack of reality feeling.

The fabulator may claim to have seen a man with three heads walking down a crowded street—while no one took notice of the man.

Nonetheless, we should give due attention to the possibility that a quite different explanation might be true. And I shall elsewhere suggest a specific alternative hypothesis. 1. The infantile event was invented by Freud in exactly the same way in which he fabricated the spying event about Dora and the fellatio event about G.de B. 2. Elsa was never ill, and was never a patient of Freud's. She was merely a friend of Desirée. 3. Freud forgot himself and inadvertently told the truth, viz. that 4. *if* Desirée and Elsa really had reciprocally confirmed each other's accounts, *then* Freud would thereby have obtained external confirmation for *one*, *not two*, out of those patients whom he really had.

§283. There are more oddities about Christina. But let us first be clear as to the one already pointed out. Let us apply a jurisprudential model.

Suppose it was until 1964 a crime to import the delta article into the country epsilon-shire; and that the period for prosecution of smuggling of delta articles will never expire. Suppose Mr. Thetabaum is tried in 1985 for having smuggled this product in 1961. In his final plea the general prosecutor eloquently boasts of having unassailable proof of Mr. Thetabaum's guilt. And then he refers to a number of testimonies of eye-witnesses who claimed to have seen Thetabaum importing the delta article at some indeterminate time during the 1970s, and also said that he had done the same thing at an "earlier" time.

If the general prosecutor had worked intensively and extensively with this question during the 171 calendar days immediately preceding his plea, and had repeated exactly the same plea in the court of appeal and the supreme court, we would certainly have reason to feel suspicious.

§284. Whatever "later childhood" may mean, it must refer to a time when Christina was more than 8 years 4 months old. But Freud's observations of the 18 patients seemed to prove that sexual seduction after this date could never have harmful effects. Then why should Christina *repress* her memory of the event?

Furthermore, *when Freud later claimed that the patients had merely fabricated the seduction events, he never bothered to explain how fabricated events could have been externally verified.*

§285. If the above statements on external verification are compared with the text of the *second* seduction paper, posted on 5 February 1896, a remarkable pattern will ensue. The second seduction paper comprises 11 female and 2 male patients.

"In seven out of the thirteen cases the intercourse was between children on both sides—sexual relations between a little girl and a boy a little older (most often her brother) who had himself been the victim of an earlier seduction. These relations sometimes continued for years, until the little guilty parties reached puberty; the boy would repeat the same practices with the little girl over and over again and without alteration—practices to which he himself had been subjected by some female servant or governess and which on account of their origin were often of a disgusting sort."

[Concerning all 13 patients:] In two cases the series started in the little creature's second year (?) [Freud's question-mark]; the commonest age in my observations is the

fourth or fifth year. It may be somewhat by accident, but I have formed an impression from this that a passive sexual experience occurring only after the age of from eight to ten is no longer able to serve as the foundation of the neurosis." (GW-I:417/SE-III:152f.) [Q-285:1]

Some aspects of this quotation will be discussed elsewhere. In April Freud had obtained a much stronger conviction as to the non-injurious effect of seduction after the age of 8. And the age of 10 is not mentioned. Likewise, the seduction events are in the third seduction paper said to have occurred when the patient was 2-4, while 4-5 is the *commonests* age for the February sample of 13 patients. It is strange that the subsample should have a greater range, on no less than two variables, than the entire sample.—We have here one more illustration of *the deficient reality feeling of the fabulator*.

When Freud talks about "intercourse" (here: "une liaison infantile", since the second seduction paper is in French), or "Coitus" (in his German texts), he does not at all mean "intercourse". Cf. §347.

§286. But now to aspects to be discussed at the present place.

Q-285:1 cannot easily be interpreted in any other way that this: Seven out of the 11 females had experienced sexual relations with their brother or some other boy, who was just *a little* older. And *they had experienced no seduction by any adult*.

As for the remaining 4 females, there is *formally* room for the interpretation that some of these had experienced sexual acts with both adults and children. But I, for one, feel myself unable to imagine that Freud would have used the formulations of Q-285:1 if more than the former 7 patients had participated in pure child sex.

§287. *How did Freud find out that the 7 seducing boys had previously been seduced by an adult female? And that the latter was a female servant or governess?*

There are a total of four ways, and I shall list all of them.

- A. The female patient told Freud these things.
- B. The boy who was likewise a patient, told Freud.
- C. The boy who was not a patient of Freud's, told him.
- D. An external observer told Freud.

None of these alternative can be maintained.

Little space need be wasted on the last alternative. Assume two sibling, Adam and Eve. When Adam was three years old, his mother caught the governess in performing a sexual act upon him. When he was five, his mother caught him in practicing a sexual act upon his three-year-old sister. Eve got ill when she was 19. Freud asked her mother for information. The mother told him about the act between the governess and her son, Adam, as well as the act Adam had performed upon a little girl two years later. But she forgot to tell that Eve, the individual who had got ill and about whom Freud inquired, was the girl involved. Hence, Freud still had to dig out Eve's recollection.

A variant of this pattern will remedy the primary oddity. Freud might have inquired about Eve *after* termination and recovery. Now, we have already seen

that Eve (if she was one of the 18 and, hence, one of the 13 patients) neither recovered nor experienced de-repression. Also, it would be completely alien to Freud's thinking to take himself much trouble of obtaining external evidence.

But if Freud had really observed 7 cases of females who had participated in pure child sex, sometimes for 7 years or more, while the boy had previously been seduced by a servant or governess, it is improbable in the extreme that external observers would be available for both types of events for all 7 patients.

§288. The February sample comprised only 2 males. There is simply no room for the hypothesis that "7 out of 2" boys had, as patients, informed Freud about their experiences. Even the April sample consisted of less than 7 males. And it contained no instance of a sexual act having been reciprocally confirmed by the agreement between the testimonies of two patients of the opposite sexes.

Under the assumption that Freud had learned about the previous seduction of the boy-partners of the 7 female patients from *anyone* except the female patients themselves, Christina could not possibly have belonged to these 7 females, since her brother had *not* confirmed any act he had performed during the *relevant* age. As for Desirée and Elsa, it is my definite, but insufficiently supported, view that Freud had a child in mind when he talked of a "male person". If this view of mine is erroneous, they might have belonged to the 7. If my view is true, they could not easily have belonged to the remaining 4 females of the February sample, since none of these seemed to have experienced pure child sex (that is, acts involving only other children). We would have to conclude that they really belonged to the 7, although it is a little surprising that Freud did not even mention the group sex acts.

If Freud told the truth (as we know he did not), Christina must necessarily have been the only female newcomer in the April sample. Hence, there is no room for the idea that Desirée belonged to the 7 females while Elsa did not; or vice versa.

§289. But whatever pattern we choose, we shall not escape a strange inconsistency. *Among the 13 patients of the February sample, Freud obtained external verification for A MINIMUM of 7. Among the 18 patients of the April sample, Freud obtained external verification for 1–3 ADDITIONAL cases. Hence, the April sample should contain A MINIMUM of 8–10 instances of external verification. Strangely, Freud forgot of 6–8 of these.*

The reader may judge for himself whether it is likely that Freud would have remained silent about the following circumstances, if he had really observed them. Among his female patients 67% had participated in pure child sex. And at least 58% had participated in no other kind of sexual activity during childhood.

§290. Let us see whether Freud will be any better off if we—at least for the moment—drop the assumption that Freud had obtained external confirmation of the boy-partners' previous sexual experiences with adult females, from the boys or from some third person. The only alternative is to assume that Freud learned about the sexual performance and social profession (being a servant, a governess etc.), from the female patients themselves.



Let us disregard the peculiar condition that a 2–4 year old girl must be capable of distinguishing between a servant and a governess. But *how* did the girl obtain the relevant information? Only two options are open. Either, the girl was an eyewitness, or she had a parapsychic capacity.

It would be a most unusually behaviour to perform the acts right under the eyes of the child. Admittedly, a governess might have taken the chance while the little girl was having a nap—and the girl might unexpectedly or even unnoticedly have woken up during the act. But is this explanation likely for *7 cases out of 7*?

As for parapsychic capacities, they do not seem to have ever been observed, except when the methodological design leaves room for cheating. However this may be, even the most ardent proponents of parapsychics have hardly ever suggested such strong effects as we need in the present situation.

§291. In other words, whether we interpret Q-285:1 as meaning that Freud obtained the relevant information about the boy-partners, from the female patient themselves, or from any other source, Freud will find himself in unsurmountable trouble. Because he was fabulating, he had completely severed the connection between his postulations and the empirical world. Hence, he had no feeling of the unrealistic nature of his statements.

If Freud had actually observed anything even remotely similar to the postulated configurations, he could hardly have failed to be highly surprised. In fact, one of the procedures by means of which the fabulator may be exposed, derives from his tendency of postulating extremely surprising patterns of fact, while manifesting a total absence of any feeling of surprise. But this subject belongs in a later volume.

What Freud has presented here is evidently a typical *one-step argument*. It will collapse by its own weight as soon as it has been spelled out completely.

§292. Additional curious patterns may be noticed. First, if the boy-partners of the 7 females were usually brothers who were living together with the sisters who eventually got ill, and if the brothers had retained a clear memory of the events, then *the criterion of independency* is not at all satisfied. Did none of the brothers try to help by informing their sister about what had happened, when she had got ill and had started treatment by a therapist who assigned an enormous importance to sexual behaviour during childhood?

A second curiosity. Freud himself implicitly states the following thing. If a female be picked up at random, and it be fabricated out of thin air that she had participated in sexual activity with her brother, then the probability is, *on account of chance*, high that the fabrication is correct. In other words, if Freud's claims about Christina had no root in any clinical observations whatever, there would be little risk in asserting them.

“I will ask you to consider a moment longer the special frequency with which sexual relations in childhood occur precisely between brothers and sisters and cousins, as a result of their opportunities for being together so often.” (GW-I:445/SE-III:209) [Q-292:1]



This circumstance does not per se prove that Freud's claims about Christina were indeed fabricated. However: Freud *first* claimed to have unassailable proof, and more or less implied that he would present a specimen. But what he did advance, was such things which, according to his own view, it would be gratuitous to assert about any unknown individual. Hence, we are entitled to be suspicious.

§293. Moreover, Christina is nowhere mentioned in the entire third seduction paper (nor in any of the preceding seduction papers), except at one place, viz. where she is needed for backing up the deliberately false claim about the existence of external verification. We have already seen that none of the 18 patients (if they were indeed 18?) claimed to have been seduced, but rather denied having been so. Recall also the facts that none of the patients had experienced any instance of de-repression, nor had they recovered.

In fact, *it is one of Freud's most recurrent techniques to put his fabrications into the mouths of other Persons*. Allegedly, Dora herself recounted the spying event (GW-V:243/SE-VII:80). Dora interpreted the fact that one of her cousins had stomach-gripes as evidence that she was a masturbator. (GW-V:241/SE-VII:78).

§294. As regards the latter example, many isomorphic relations to the case of Christina may be noted. Once more, we are concerned with a statement attributed to Dora, which it would be gratuitous to fabricate:

"It is well known that gastric pains occur especially often in those who masturbate."  
(GW-V:241/SE-VII:78) [Q-294:1]

Once more, Freud's view as to what kind of facts will *usually* pertain in the empirical world, is so erroneous, that it could never have been derived from any genuine observations.

Once more, the statement that is put into the patient's mouth, is advanced exactly at the point when it is *needed*, viz. for proving that Dora herself had masturbated. Allegedly, Dora 1. denied having masturbated, 2. admitted of suffering from stomach-gripes, 3. took the "fact" that her cousin had stomach-gripes as evidence of the latter's masturbation.

I may remark in passing that the view that stomach-gripes and masturbation are closely correlated, was shared by Wilhelm Fliess:

"Among the pains which derive from masturbation, I would like to emphasize one in particular, because of its importance: neuralgic stomach pain." (Fliess: *Über den ursächlichen Zusammenhang von Nase und Geschlechtsorgan*, 1902:7, quoted in Masson, 1984:77) [Q-294:1]

According to Masson, Fliess entertained this view since 1893, and the latter was no doubt a prominent subject of the conversations between Freud and Fliess in 1894 and 1895. Hence, Freud may (or may not) have learned the idea from Fliess. However, it was by no means Fliess who invented this prejudice. Gattel (1898:25) had a patient who suffered from a chronical similar symptom at the age of 20–29, that is during 1869 and 1878. A doctor in Prag named von Jaksch had explained this syndrome as the result of masturbation performed until he was 15.

As so often, Freud uncritically adopted traditional prejudices.

§295. Already in Q-16:1 we observed what kind and degree of acrobatics Freud needed, in order to fabricate any evidence of Dora's alleged love of Mr. K. No unbiased person would find *any* trace of such love. And it is improbable in the extreme that two individuals would, independently of each other, arrive at such a far-fetched idea and, afterwards, engage into some kind of interaction. Hence, it is hardly possible that Dora's cousin really said: "Why you're simply wild about that man!" (= Mr. K.) (GW-V:196/SE-VII:37).

§296. A last example from the case-study of Dora. Allegedly, Dora took the fact that her mother suffered from white vaginal discharge as evidence that she had syphilis and had been contaminated by her father. Dora had likewise overheard a statement by her aunt referring to her father's [alleged] syphilis: "He was ill before his marriage, you know." (GW-V:237f./SE-VII:75f.)

Many comments are appropriate here. First, immediately after this account Freud explains that white vaginal discharge in young females (and I guess Freud thinks of virgins) is caused by masturbation. In *Drei Abhandlungen zur Sexualtheorie* Freud explicitly and directly compared bed-wetting with nocturnal seminal emissions (GW-V:90/SE-VII:190). And he "proved" Dora's childhood masturbation from the [alleged] fact that she had suffered from bed-wetting (GW-V:236f./SE-VII:74). It is evident from Freud's text that *he himself* conceived of the mother's [alleged] white vaginal discharge as the result of syphilis. But it is certainly not likely that this curious idea would occur to any other individual. As for the few persons who might have entertained the same view, it is improbable in the extreme that Freud would ever have the opportunity of meeting any of them.

§297. Second, just like Freud derived childhood masturbation and infantile seduction by means of *the principle of similarity*, so he was very much concerned about syphilis. The rat man suffered from a compulsion involving rats. Freud added the following deduction of his own. Rats carry infectious diseases, and syphilis is an infectious disease. And these three circumstances prove that the rat man unconsciously entertained the suspicion that his father had in his youth lived an amoral sexual life (GW-VII:433/SE-X:214).

Freud (GW-V:177/SE-VII:19) claims that Dora's father had confessed to having contracted a venereal disease before he married. Felix Deutsch (1957: 162) states that Dora had "*forgotten*" the fact that her father "had" syphilis, when he met her at the age of 39-40.

§298. The statement by Dora's aunt is by only a few lines preceded by the following sentence: "It turned out, TO MY ASTONISHMENT, that the girl knew what the nature of her father's illness had been." (GW-V:237/SE-VII:75)

As we have already seen, Freud repeatedly asserts the most astonishing postulations, without experiencing the least sense of astonishment. Likewise, he again and again FEIGNS to be astonished BECAUSE OF PERSUASIVE REASON. These instances will be analyzed in a later volume. But I doubt very much that one single instance can be found in *Gesammelte Werke*, in which Freud claims to be surprised, without there being strong or conclusive evidence that Freud was

not in the least surprised. The surprise claim is generally used as a *persuasive preamble* aiming at distracting away the reader's attention from one of two patterns: either, the presented observations are deliberately fabricated; or else, these observations are devoid of any evidential power.

It is as if Freud said: "Look, reader, I could not have faked these observations, because they run counter to my expectations. Consequently, reader, you are obliged to accept that I really made the observations."—Or alternatively: "I never dared hope to obtain evidence of such a strong power. Therefore, reader, you must admit that the evidential power of these observations is indeed impressive."

Often, such stratagems lead to uncritical acceptance of the persuasive message by the academic community.

At the present stage, *my* reader is not asked to accept anything more than *the possibility* that the surprise claim about Dora's alleged knowledge of her father's alleged illness, is a persuasive device. Genuine support will be supplied in a later volume.

§299. It must however be noted that: the claims about Dora's and her mother's vaginal fluid are strongly *imbedded* in a section in which Freud is eager to prove that Dora masturbated during the latency. And each and all proofs in this section consist of curious logical derivations, together with the ad hoc fabrication of empirical generalizations, to each of which is attached the claim that it has previously been amply verified by Freud.

In a later volume I shall discuss *the criterion of imbeddedness* as a ground for the conclusion that an observation statement is deliberately faked.

But what must be mentioned at the present place, albeit only briefly, is *the criterion of simple isomorphy*. The latter is identical with what Trankell (1971) merely calls "the criterion of isomorphy". I have re-christened it, because I shall eventually add a "*criterion of lateral isomorphy*". The basic idea of Trankell's criterion is this. Let the following conditions be given. a. Two statements, S:1 and S:2 are uttered by the same individual. b. S:1 and S:2 are isomorphic. c. S:1 is known to be empirically false. d. The individual is known to have been aware of lying when he uttered S:1. Then we are entitled to conclude that S:2 is *probably* likewise deliberately false.

§300. One difference between Trankell's and my positions is much less than it might appear. Trankell's conclusion is said to be merely probable, while I have skipped the modal qualification. However, Trankell is primarily concerned with cases in which *one* conclusion is based upon *one* premise (= *one* statement made by the individual in question). My conclusion will almost invariably be based upon *numerous* premises—10 or 20 or many more. It seems clear enough that the condition of independence *in the relevant respects* is indeed satisfied. *Unless* my conclusion is true (that is, unless a circumstance pertains, which *cannot* appear as a premise), the fact that Freud fabricated the spying event, should neither increase nor decrease the probability that he would also fabricate the group sex events allegedly recounted by Desirée and Elsa.

I shall take no stand as to what variety of calculation upon the probability fig-

ures may be most appropriate. If normal multiplication is applied, the probability of being mistaken will soon decrease to a microscopic level.

Let the probability of being mistaken be 0,33 if the conclusion that S:2 is deliberately false, is based on S:1 alone. Then the same conclusion based upon 5 comparable but independent statements, will be 0,004.

Trankell generally works with much greater probability figures. Hence, my figures would be truly microscopic, even if I had no more than 5 premises for each argument.

Much more may be said about the method of drawing inferences on the basis of the relation of isomorphy, and will be said later. But I think we may safely conclude at the present stage that the external verification related to Christina is deliberately fabricated.

§301. As for Desirée and Elsa, we have Freud's own testimony in his private letters, according to which none of the 18 patient experienced any instance of depression. We also have Freud's testimony in the published text, according to which the patients *denied* having been seduced. Hence, the postulation of external verification because of the agreement of their accounts, is fabricated.

Note how all the different lines of reasoning *converge* in the same conclusion. Flagrantly, the external verification is altogether faked.

§302. I shall now explain the contradiction between the second and third seduction papers, as to whether the critical age after which sexual seduction can have no harmful effects, is 8 or 8–10.

I have already in §9 explained the derivation of the lower age. But Fliess assumes the existence of a *male* cycle of 23 days and a *female* cycle of 28 days. While  $23 \cdot 12 \cdot 12$  days after conception will yield about 8 years 4 months after birth,  $28 \cdot 12 \cdot 12$  days after conception will yield about 10 years 3 1/2 months after birth.

It is a little surprising that the period of progestation should be related to the *male* period. But Freud's postulations about his patients have no foundation in any clinical observations. The reader may decide for himself whether the following sentence should be construed as a purely persuasive device to conceal the total absence of evidence, or as a reference to magical numerology. Clearly, there is no further alternative.

"I should accuse myself of blameworthy credulity if I did not possess more conclusive evidence" [than the accounts presented by the patients]. (418/SE-III:153) [Q-302:1]

It is easy to understand why Freud did not present the numerological proof in print. However, in the third paper Freud explicitly claimed that *the accounts* presented by the patients contain absolutely conclusive evidence. Consequently, there would not be the slightest hint of "crédulité blâmable", even if Freud had no other evidence.

"[...] another and stronger proof of this [viz. of the reality of the infantile events] is furnished by the relationship of the infantile scenes to the content of the whole of the rest of the case history. It is exactly like putting together a child's picture-puzzle: after many attempts, we become absolutely certain in the end which piece belongs in the

empty gap; for only that one piece fills out the picture and at the same time allows its irregular edges to be filled into the edges of the other pieces in such a manner as to leave no free space and to entail no overlapping. In the same way, the contents of the infantile scenes turn out to be indispensable supplements to the associative and logical framework of the neurosis, whose insertion makes its course of development for the first time evident, or even, as we might often say, self-evident." (GW-I:441f./SE-III:205) [Q-302:2]

This text will be extensively analyzed in a later volume, when we shall discuss *the method of the jigsaw puzzle*; the reader may satisfy himself that the biography of Victor (presented in §§376ff.) does indeed have the degree and quality of evidential power postulated by Freud. At the present place we may merely notice the contradictions between Freud's assertions as to the nature of his evidence.

§303. There are strong reason to analyze at this place a number of features that are not directly related to the problem of external verification, but that are closely related to the same sections of Freud's papers.

Recall from §285 that "the relations continued for years, *until* the little guilty parties *reached* puberty" (GW-I:418/SE-III:152, italics added). However, in the *third* paper we are instead told that it is a matter of "sexual relations between two children of different sexes, mostly a brother and sister, which are *often prolonged beyond puberty*" (GW-I:444/SE-III:208) italics added).

In ch. 3 I developed and supported the hypothesis that each fabrication changed Freud's "adaptation level" and, consequently, gave him courage to assert progressively more bold fabrications. Let us see whether there is any possibility of escaping the conclusion that this mechanism was responsible for the inconsistency at hand.

It is immediately seen that the only way out must be related to the fact that the April sample contains 5 newcomers.

There is only one female newcomer, viz. Christina. Her brother confirmed acts performed during her "later childhood", but *not* beyond puberty. Hence, the only procedure for making a positive instance out of her case, is to assume that she had had an additional partner who had seduced her when she was 2–4 years old (or 2–5, according to the second paper), and had continued the relation beyond puberty. It does not seem that Freud had any such sequence of events in mind.

§304. The two *males* of the February sample could not easily have participated in pure child sex. Hence, we are left with only the 4 male newcomers. As I shall show below, Freud's text requires that at least 3 out of these, and possibly all 4, had participated in pure child sex.

To save Freud's integrity we must assume the following pattern to pertain. a. In the February sample Freud observed pure child sex solely among the females. b. Among the newcomers to the April sample Freud observed pure child sex solely among the males. c. SOME of the 7 February females had continued the child sex relation *until* but not *beyond* puberty. d. OFTEN the pure child sex relation was prolonged *beyond* puberty within the group consisting of *both* the 7 February females and the 3–4 male newcomers, that is among 10–11 patients. e. Among



both the 7 February females and the 10–12 April patients of mixed sex, the pure child sex relation most often involved a brother and a sister.

Evidently, it is improbable that all or nearly all male newcomers had practiced pure child sex beyond puberty; that Freud would encounter 3–4 *consecutive* cases of this character; while all cases encountered prior to a certain date, whether female or male, were of a different nature.

Moreover, even if each and all 3–4 male newcomers had practiced pure child sex beyond puberty, 3 out of 10, or 4 out of 11, are hardly proportions which would justify the word “often” (“oft”).

While the pattern described by Freud cannot be said to be formally impossible, it is so odd, that the hypothesis is vastly more probable that Freud, as so often elsewhere, had no feeling of the strange nature of his account, exactly because he was fabulating.

There is a close analogy between the present instance and the biography of Florence, cf. §§336ff. In both cases a configuration *can* be constructed, which is compatible with all circumstances asserted by Freud. In both cases this configuration is so extraordinary that it is extremely difficult to believe that Freud could have observed any such configuration without having felt the need to explain to the reader the highly unusual facts needed to make the accounts non-contradictory.

§305. One more strange problem. We are told that:

“Before they come for analysis the patients know nothing about these scenes. [...] With our patients, those memories are never conscious.” (GW-I:440, 448/SE-III:204, 211) [Q-305:I]

Now, suppose the following concrete sequence of events. a. Irma was seduced by her brother for the first time when she was 4 years 4 months old; he was 5. b. These sibling performed about one act a week or, say, 50 acts a year. c. More specifically, they performed 200 acts until Irma reached the critical age beyond which such acts are no longer harmful. d. They performed 200 further acts beyond the critical age and until Irma reached puberty.

*At what time did repression take place? And: how many acts of repression did Irma perform?*

It would certainly be a queer idea that she repressed each and all sexual acts, one at the time, but nonetheless was prepared to engage in the same kind of sexual act a week later.

At the opposite extreme, she might have been perfectly conscious of every sexual act until she was 12 (or 13 or 14). And then she might in one stroke have repressed all 400 sexual acts. But since the last 200 acts were not harmful at all, *why* should she repress them? And *why* should she repress at that time an activity which was painful to such a limited degree, that her mind felt no need to repress it during 8 years.

§306. The former inconsistency may be resolved, if we assume that Irma repressed the *first* 200 sexual acts in one stroke when she was 8, but never re-



pressed the *last 200 acts*. (The second difficulty is by no means remedied.) However, this assumption will necessitate a re-interpretation of Q-305:1 according to which Irma was perfectly aware of the last 200 non-pathogenic acts. But before she came for analysis she knew nothing about any of the preceding 200 scenes.

I doubt that any reader will suggest that this is what Freud meant, and that he conceived of Q-305:1 as appropriate formulations for communicating *this* message.

This re-interpretation will produce more problems than it will solve. First, it will entail that Freud was capable of ascertaining whether Irma during a certain consultation recounted the 201th or the 199th sexual act. In both cases she would recount the identical sequence of events. She would “have gone through [it] once more in such a convincing manner” and “in such a way that [she] seemed to be living through it with all the appropriate feelings”. As for the features of the sequence of events, it would possess “the relevant *suitability to serve as a determinant*”. (GW-I:440, 441, 428/SE-III:204, 205, 193; Freud’s italics)

§307. Now look at the following formulations: the repressed memories can only be made available to the patient’s conscious mind by “the strongest compulsion of the treatment”, “under the most energetic pressure of the analytic procedure”, “against an enormous resistance”, and “must be extracted from them piece by piece” (GW-I:440, 418/SE-III:204, 153).

*When Irma had already recounted 100% of sexual act no. 201–400; and 25% of the pieces of act no. 199 had been extracted; how would the embryo of act no.199 look?*

§308. Next, I shall turn to one of the contradictions in Q-14:1,

s-3 *A not indicated number among the 18 patients had (at the age of 2–4) participated in sexual actions involving solely other children and no adult. Freud verified the fact that each and all of these children had previously been seduced by an adult person. And within the pure child relation, the child who initiated the relation had repeated exactly the same variety of sexual action which the adult had performed upon him or her.* (GW-I:452/SE-III:215)

s-4 *A not indicated number of THE BOYS had participated in such sexual actions involving only other children. Although Freud succeeded in digging out all seductive events there might have been: for some of the boys who had performed pure child sex he found no previous seduction by any adult.* (GW-I:445/SE-III:208)

[Q-308:1]

We have already seen that this contradiction is related to the contradictions around the alleged external verification. I shall quote Freud’s own texts.

“Where the relation is between two children, the character of the sexual scenes is none the less of the same repulsive sort, since every such relationship between children postulates a previous seduction of one of them by an adult.” (GW-I:452/SE-III:215) [Q-308:2]

"Where there had been a relation between two children I was sometimes able to prove that the boy—who, here too, played the part of the aggressor—had previously been seduced by an adult of the female sex, and that afterwards, under the pressure of his prematurely awakened libido and compelled by his memory, he tried to repeat with the little girl exactly the same practices that he had learned from the adult woman, without making any modification of his own in the character of the sexual activity." (GW-I: 445/SE-III:208) [Q-308:3]

§309. Regrettably, I shall have to refute a few odd attempts at explaining away the contradiction between Q-308:2 and Q-308:3. First attempt. In Q-308:2 Freud merely formulates *a theory to be tested*, and in Q-308:3 we are told the outcome of the test. There is nothing remarkable about a factual relation being postulated by a theory to hold universally, but being empirically observed to hold only in some cases.

But the context of Q-308:2 leaves no room for more than one interpretation. Freud *observed* that all children who had initiated pure child sex, had applied the "repulsive" variants, such as fellatio, anal finger masturbation etc. He explains that: a little boy is necessarily impotent; hence, the option of normal coitus is not open to an adult female who seduces a little boy. And adult males who are prepared to seduce young girls, are either impotent or else so depraved that they will only practice other variants than coitus [!]

Besides, Freud has unambiguously asserted that he did *not* start out with an abstract theory to be eventually tested. On the contrary, his theory is little more than a summary of his clinical observations.

§310. Now to the second attempt of resolving the contradiction. If Freud's statements are taken at face value, the two males who are common to the February and the April samples had not participated in pure child sex. Either 3 or all 4 of the newcomers had done so. But for only *some* of these (2 or 3) had Freud disclosed previous seduction by an adult—hence, such prior experience had not been disclosed for 1–2 males. And according to the objection, there is a difference between the statement that prior seduction had not been *ascertained* for a certain male, and the postulation that he had not *experienced* prior seduction.

The trouble with this objection is that Freud again and again emphasizes that he succeeded in digging out all repressed seduction events. But the objection may be modified. 1. One of the patients, say, Otto had experienced prior seduction. 2. Otto had not repressed the event. 3. Otto was treated for a prolonged time, probably for at least 100 hours. 4. Freud obtained thousands of details forming the most closely knit completed picture of a jigsaw puzzle, of the de-repressed seduction events and their relation to Otto's entire life experiences. 5. Freud was evidently highly interested in the question of prior seduction by adults, of children who had initiated pure child sex acts. 6. Freud learned about Otto's seduction of, say, his sister, when he was 4 and she was 3. 7. Freud completely forgot to ask about any prior seduction, and Otto completely forgot to tell anything about the matter.

I frankly admit that I am unable to rebuke *this* objection. But I do not think I am under any obligation of doing so.

§311. We shall now turn to an objection which more directly fits in the present chapter, viz. that Q-308:3 is really about the non-patients who were the child partners of some of Freud's female patients.

But the arithmetics does not tally. Freud had disclosed prior seduction for each and all child partners of the 7 females of the February sample who had participated in pure child sex. The remaining 4 females had no such experiences. Christina was the only newcomer. We would therefore have to conclude that her brother was the only partner about whom prior seduction was not disclosed.

It would be awkward to say in English "I was SOMETIMES able to prove", if proof had been obtained in 7 instances out of 8. And it is altogether impossible that Freud could have used the German phrase "EINIGE MALE" in this sense.

§312. I myself think that Freud, because he was fabulating, had confused idea as to *what* kinds of boys he was talking about. But nothing hinges upon whether I am correct in this respect.

No one who has read the third seduction paper alone, could possibly take Q-308:3 to refer to anything else than Freud's own male patients.

Moreover, Freud eventually presents a theory of the etiology of the compulsive neurosis, which is simply not compatible with the idea that Q-308:3 is about non-patients. According to this theory:

Both hysteria and compulsive neurosis are caused by sexual activity during early childhood, but in different ways. If the child is the seducer and, hence, will experience the act with enjoyment, he or she may later feel remorse. And this subsequent reaction may eventually be transformed into a genuine compulsive neurosis. If the child is merely the passive victim of seduction and, hence, will experience little enjoyment, hysteria is likely to ensue. (GW-I:457f./SE-III:219f.)

This causal relation will, according to Freud, explain the fact that males will more frequently suffer from compulsive neuroses, while females will more frequently suffer from hysteria.

At least one point may need clarification. The males who had initiated pure child sex acts, had *previously* been the passive victim of seduction by some adult person. Wouldn't it be a natural expectation that their *first* experience should have the greatest causal effect? If so, how could a compulsive neurosis ever emerge?

Basically, there are two possibilities. He may really have enjoyed the act, despite the fact that he was merely a passive victim. Alternatively, the act may have been painful, but the boy might hope to overcome the trauma by re-creating an analogous situation. The latter alternative will produce no trouble for Freud. However, in Q-308:3 he talks of the boy's "prematurely awakened libido", and this formulation clearly reveals that Freud had the former alternative in mind. Consequently, even a passive victim may feel enjoyment. But if this be the case, Freud is faced with the task of explaining why a passive victim could not have felt remorse, which would eventually be transformed into a compulsive neurosis. And why should this psychodynamic pattern not be applicable to young girls and boys who had solely been seduced by an adult?

§313. In the case-study of Dora, the patient remains completely unknown and anonymous. Freud has merely imputed a patient stereotype upon her. This stereotype is possessed by all kinds of normal and less normal sex, and is indiscriminate as to the partner. The reader may judge for himself whether the same patient stereotype is alive here.

But since females may occasionally develop a compulsive neurosis there must, on Freud's premises, be some young girls who have seduced young boys. To do what?

The adult seducer did not practice normal coitus. (And even if he did, how would the girl have managed to make a pre-school boy repeat this kind of act?) But if the adult practiced fellatio, or anal finger masturbation, it may not be altogether easy for a very young girl to seduce a young boy into doing the same thing upon her. It is even less understandable why a young girl should wish to have such acts repeated.

In a footnote (GW-I:440/SE-III:204), written in 1924, Freud recounts his clinical experiences during the 28 years subsequent to the publication of the third seduction paper. He claims that hysterical patients behave exactly as he described in 1896. (And then he adds that his mistake derived from the fact that he had believed that the patients told the truth.)

But he forgot one very important thing. *Do compulsive neurotics also behave exactly as Freud described in 1896? Do they also start to fabulate about having been seduced and then having seduced another child, exactly before they lose their symptoms?*

Since the compulsive neurosis is closely connected with the anal theory, should we guess that they fabulate about anal finger masturbation and such things?

§314. This conglomeration of both transparant and non-transparant contradictions has held a world in admiration for generations. No criteria for identifying deliberate fabulation are needed in order to see that Freud's clinical observations are deliberately fabricated. The patients were not 18 in number, none of them were cured, none experienced de-repression, and none claimed to have been sexually seduced. Likewise, no instance of external verification was obtained by Freud.

## Taking a Closer Look at the Patients' Symptoms

*A few days later he was in a third monastery shown a box very much greater than the first one. "This is the skull of Saint Kyrikos," said the sexton. Father Yannaros could hardly keep back his surprise. "Why, but the other day I was shown the skull of a child and told that that was that of the saint. "Oh," said the monk. "Then it must have been the skull of the saint as a little one."*

Nikos Kazantzakis

§315. In contrast to almost all other commentators, I have framed a distinct separation between the methodology which Freud and his followers claim to apply, and the methodology really applied by them. An entire book will be devoted to each of these subjects. But a few words on the real methodology need be said now, and primarily about *the principle of similarity*.

This is the corner-stone of the psychoanalytic technique of interpretation. None of my readers will be able to find any interpretation in any psychoanalytic writing, which has not been deduced by means of this rule.

As for the root of the principle in traditional superstition, I shall in due course list many illustrative examples; I have already mentioned the idea that a hare-lip is caused by the pregnant mother having been scared by a hare. One more example, taken from the prison memoirs of Leonora Christina Ulfeldt (1900:46). If you want to sow dissension between two persons, you should collect hair fallen to the ground where two black cats have been fighting. And you should throw this hair between the persons.

§316. Let us juxtapose some of the analogous psychoanalytic instances we have already encountered:

1. G.de B. suffered from oral eczema. This observation proves that her father [and not just an as yet unidentified adult male] had practiced fellatio upon her when she was an infant.
2. Dora suffered from cough attacks. Since cough attacks are rhythmic like sexual intercourse, and related to the mouth like fellatio, Dora's cough attacks prove that she wished to practice fellatio with Mr. K.
3. When children relapse in bed-wetting after having been dry for some years, this symptom is usually caused by masturbation—since bed-wetting is similar to nocturnal seminal emissions.

4. When young females (virgins?) suffer from white vaginal discharge, this symptom is usually caused by masturbation (since the vaginal fluid is similar to male semen).

[Q-316:1]

§317. As I have said: in the present chapter we shall primarily focus upon the principle of similarity. Nonetheless, a single paragraph will be devoted to the specific psychoanalytic attitude to methodology. The fact should not be overlooked that psychoanalysts are aware that most readers would consider them pseudo-scientists, if they told the truth about what kinds of procedures they apply.

Rapaport (1965:141) writes: "This makes it urgent to reinvestigate Freud's case studies with the aim of clarifying whether or not they can yield a canon of clinical research at the present stage of our knowledge." Evidently, they can. And the canon will have to comprise such principles as the ones listed in Q-762:1 and, partially, in Q-186:1. However, the case-studies are not superior to the isolated interpretations throughout *Gesammelte Werke*, since these studies consist of little more than mechanical juxtapositions of isolated interpretations.

Rapaport himself has applied the psychoanalytic canon for a lifetime. Although a personal psychoanalysis may significantly reduce an individual's knowledge of himself, it is not easy to believe that Rapaport was not really aware of what he did.

§318. When Freud talked of "the difficult technique of this therapeutic procedure" (GW-I:427/SE-III:193), he was aware of not telling the truth. Freud's interpretation technique is so extremely simple, that he would never need to see the patient at all, in order to produce exactly the same "post-dictions" of the latter's childhood experiences.

In fact, any reader may as a matter of routine reconstruct Freud's deduction procedures. And the reader may easily apply them to fresh instances. To anticipate a result from a later volume: I fail to understand how Maryse Choisy could publish her cat dream without the interpretation Freud gave to her in the consultation room; and how she could think that this interpretation is not on the surface for anyone to infer.

§319. Michael suffered from a stiff leg. Now, a leg has an oblong form—like a penis. It has almost the "right" position at the male torso. In German, there may be a slightly obscene flavour of talking about a "stiff" penis, but the expression would be easy to understand in any language. Consequently, Michael's stiff leg must have been used where a "stiff" penis is normally used.

*Freud always reacted to patient symptoms and patient accounts like certain testees will react to Rorschach inkblot cards: he saw sex organs and sexual phenomena in every stimulus.* Moreover Freud solely inferred the most coarse sexual phenomena. Limited knowledge of the patient's *sexual* emotions, longings, and behaviours are needed for producing such interpretations, as well as of his non-sexual emotions and behaviours, his personality and life situation.



§320. Obviously, Freud did not need any external source of inspiration in order to arrive at his interpretation of Michael's symptom. I shall nonetheless present a source from which he may or may not have got the idea.

In Christa Wolf's (1982:481) postscript to Bettina von Arnim's novel *Die Günderode* the following text is found:

"Beseligt durch eine Selbsttäuschung gibt sie preis, was sie einst, als Zweiundzwanzig-jährige, geistig-sexuell geprägt hat, treibt einen fetischhaften Kult mit einem gipsernen Jünglingsfuss an ihrem Bette." (Wolf, 1982:481) [Q-320:1]

I take this text as a polite way of communicating that Bettina masturbated with a plaster-figure of an adolescent's foot when she was 22. More importantly, it can hardly be doubted that Freud would have understood the text in this sense. The essential question is therefore whether he knew about the fact alluded to by Wolf.

Regrettably, Wolf does not indicate her source, and I am therefore not in a position to judge how widely known the fact might have been. But there is no apriori ground for taking it as a private secret. Among Bettina's letter novels, both *Die Günderode* and *Goethe's Briefwechsel mit einem Kind* are to an astonishing degree frank and undisguised although, formally, they do not pretend to be non-fictional autobiographical works. Perhaps non-verbosely, I do not recall any reference to a plaster-foot in these two letter novels.

Bettina herself cannot easily have been unknown to Freud. Around the middle 19th century she was an idol of the liberal students, who in 1840 performed a torch-light procession to honour her. Her literary contributions as well as her personal relationships might ensure that she would not easily sink into oblivion. Inter alia, she wrote the first non-trivial social-political work in German, and was appreciated by Karl Marx. It has been suggested that she has produced the best letter-novels existing in German—an interesting suggestion in view of the fact that even Goethe has written prominent letter-novels.

Her brother was Clemens Brentano, and she was married to Achim von Arnim, the two co-editors of *Des Knabens Wunderhorn*. We may safely assume that Freud was familiar with this work, as well as with Brentano's fairy-tales. She was a friend and possibly the mistress of Goethe, and the most close friend of Karoline von Günderode who, until recently, was less known because of her literary contributions, than because of her tragic love story involving Friedrich Creuzer. C.G.Jung's system could never have been constructed unaided by Creuzer's *Symbolik und Mythologie der alten Völker*.

Future historical research may be facilitated by the above account. But the validity of my own textual and other analyses is independent as to whether Freud was inspired by Bettina.

§321. There are many other oddities about Freud's account around Michael. *How did the adult female make him comply?* Did she threaten to give him a severe spanking, or did she even give him one? Did she reward him with chocolate? Or did she succeed in presenting the whole act as having fun at "playing the puff-puff-train"?

Freud insists that the infantile sexual event must be highly traumatic in order to produce hysteria. But whether or not the seduction was traumatic for Michael, would depend on all those particular circumstances that are *not* described in Freud's article. Perhaps it never occurred to Freud that the degree of traumatic experience or effect might depend upon other circumstances than the sexual act as such. Or perhaps he revealed the deficient reality feeling so typical of fabulators.

The latter mechanism seems to be the only one which could explain another

circumstance. If a 2–4 year old boy is left on his own to do the relevant movements, the adult female cannot have much pleasure from the act. If Freud had been less ignorant of human nature, or had had a less deficient sense of reality, he would rather have fabricated that the adult female had applied her own hand and merely used Michael's foot as a masturbation tool. But even then, it would have been a formidable task to make Michael relax, so that he would not obstruct her own movements.

In this respect I have a unique experience. I have during decades played local children's songs on the piano with the index finger of children from all over the world (Korea, Saudi Arabia, Japan, Iran etc. included). These children were aged from 1 upwards. When the child recognizes the song and tries to help, it is somewhat difficult to avoid hitting the wrong keys.

§322. *How did Freud ascertain that Michael was not suffering from a purely somatogenic symptom?* Freud was an incompetent diagnostician;—and this fact alone would almost guarantee that he could not have been a successful therapist (as we know he was never during any period of his life). An unbiased reader would find it difficult to believe that any of Dora's somatic symptoms were not somatogenic. Freud hardly even noticed the fact that both Dora and her father suffered from *pulmonary* syndromes. (And not only her father had TBC, a disease with an indisputable hereditary component; her mother eventually died from TBC.) Only in one place in the case-study is there any reference to this circumstance. And Freud immediately invented the explanation that Dora had got her pulmonary symptom because she had *identified herself* with her father.

Not only is *the principle of similarity* prominent here; so is *the psychoanalytic standard operation procedure*, cf. Q-186:1, to say nothing of *the illusion of separation*. Each and all real or possible causal relations which might have been responsible for Dora's pulmonary symptom (and among which the true causal relation apparently may be found), are assumed to be non-existent, *except* the one Freud is eager to prove.

Again and again Freud and his followers dogmatically assert that only this or that construction could explain this or that observation. (Only psychoanalytic identification could explain why *both* Dora and her father had pulmonary ailments.) Such instances must be given due weight when the task is to evaluate the evidential support and the theoretical need of the psychoanalytic theory of defense mechanisms.

§323. Likewise, Dora's dragging foot, deriving from a fall down the stairs gives every indication of being an instance of tendinitis with complete degeneration of the tendon. (Henke et al., 1929, Hohmann et al., 1961) Freud completely overlooked the very possibility of this etiology (*the illusion of separation*), and applied *the principle of similarity* together with *the psychoanalytic standard operation procedure* to prove that Dora had made "a faulty step" at the seduction attempt, in wish if not in deed.

§324. In §320 I mentioned *Clemens Brentano's* influence upon Freud, and I shall say

more about this phenomenon in a later volume. Certain other aspects are most relevant at the present place.

It is a sheer impossibility that Freud was not highly familiar with the poem "*Wenn der lahme Weber träumt, er webe*", in which Brentano gives a most transparent presentation of the theory that the content of dreams is wish fulfilment. The lame weaver will dream he is weaving, the sick lark that it is flying, the mute nightingale that it is singing etc.

This poem may be found in *The Oxford Book of German Verse* (1967:252). Still more important is another poem named *Traum* ("Dream") (Brentano, 1914, I:55f.) The man dreams about his beloved incessantly washing her foot to clear away a faulty step committed in her youth. But something wrong remains with her leg. There seems to be a poetic hint of some anatomical defect.

"Sie sprach: 'Mein Fuss, der wird wohl rein  
Durch dieses Wassers Tugend,  
Doch ach, stets bleibt das Oberbein  
Vom Fehltritt in der Jugend.'

[...]

Sie sprach: 'Wohin, wohin nun ziehn?  
Ich hab' den Fuss vertreten.'

(Brentano, I, 1914:55f.) [Q-324:1]

§325. In the biography of Michael in §243 we are told that Michael had, prior to the emergence of his paraplegia, paid much attention to the function of his legs. It is a recurrent phenomenon that somatogenic diseases may manifest themselves during years or even decades, as nothing but minor dysfunctions, pains, or sensations—until they eventually blossom forth. Freud applies *the illusion of separation* and, on apriori grounds, takes this causal relation to be non-existent. He talks of Michael's "*neurotic*" concern with his legs.

§326. Freud (GW-I:425, 456/SE-III:191, 218) expresses his contempt of the patient's own ideas concerning the etiology of their symptoms. Just because attacks of vomiting emerged after a railroad accident, the patient may fancy that his attacks were *caused* by the accident. Freud supplies an explanation as to why such ideas are preposterous. *It is an aprioristic axiom that the cause must be SIMILAR to the effect. But there is no SIMILARITY between a railroad accident and attacks of vomiting. CONSEQUENTLY, the accident cannot be the cause of the vegetative symptom.*

Centuries before Freud, it was a well-known fact that non-specific vegetative reactions, inter alia vomiting, are very often produced by intensive stress, e.g. in prisoners who are about to be hanged, or in soldiers under fire.

§327. In the case-study of the rat man Freud himself asserts the very opposite generalization to be universally valid. One will find the cause of a compulsive idea or behaviour, by disclosing under what circumstances the idea or behaviour made its first appearance, as well as its later occurrences. (GW-VII:409/SE-X:186)

Without noticing the contradiction, Freud asserts in the very same case-study that the patient's unconscious will interpolate an interval between the pathogenic situation (the cause) and the resulting compulsive idea, for the purpose of camouflaging the true cause. (GW-VII:461/SE-X:246)

There would be nothing remarkable about the suggestion that cause and effect will sometimes show temporal proximity, sometimes be separated by a rather brief interval, and sometime be separated by years. It is a quite different thing to *prove* a causal relation *by means of* a principle that is postulated to be universally valid, and then to reject the same principle whenever the latter will leave no room for one's prejudices.

But recall also that Freud is very prone to fabricate temporal relations.

§328. Moreover, the idea of proving causal relations by means of temporal proximity would be legitimate as *an empirical generalization*, or as a *heuristic rule*—but not as *an aprioristic principle*. The world is replete with strange coincidences, and I shall supply one illustration. I defended my doctoral thesis at Uppsala on 4 June 1984. It is the first Swedish thesis that has been printed in Cyprus. On 3 June the director of the printing company arrived in Sweden for the first time in his life, and on 5 June he went back. But he had no thought of listening to my defense. He bought a machine from a bankrupt Swedish printer.

§329. Gattel (1898) will be extensively and intensively discussed in vol. II, and we shall throw a glance at his research later in the present chapter. He applies the diagnosis “hysteria” (inter alia) if the patient has experienced a physical traumatic event (such as a fall on her head), without any subsequent *noticeable* anatomical lesion. This is an illegitimate deduction. Even today, nearly a century later, many anatomical lesions the effects of which are most palpable, are too small to be detected. (Cf. in particular the case of Johanna K. in Q-362:2.)

§330. Could any symptom be imagined, for which no infantile seduction event that is SIMILAR, could be suggested? Possibly, but for numerous symptoms it is a matter of routine to suggest such events. A few examples.

Stiffness, feebleness, jerkings, pain, hyper-sensitivity, insensitivity, inflammation etc., of a leg or an arm (or a part of either). *Interpretation*: this member was used as a masturbation tool during infancy.

Haemorrhoids, constipation, diarrhoea, inability to hold back the faeces, pain in the anus. *Interpretation*: anal masturbation or intercourse [was practiced upon the patient during infancy].

Pain, insensitivity, inflammation, white discharge etc. related to vagina. *Interpretation*: vaginal masturbation or intercourse, cunnilingus.

Oral eczema, loss of taste, loss of speech, cough attacks, vomiting. *Interpretation*: fellatio.

Insensitivity or pain on any surface area of the body. *Interpretation*: this area was the object of fondling, pinching or smacking etc., and these activities were either the primary focus of the seduction act, or were performed concomittant with some other act.

Blindness or partially impaired vision. *Interpretation*: self-punishment because the child had enjoyed seeing forbidden things during the seductive act. (The act might have consisted of the seducer exhibiting his body while masturbating.) [Q-330:1]

Some of my readers may object that no psychoanalyst would ever make such interpretations. Apart from the fact that quite a few instances have been quoted above, many of these critics have no doubt read the text upon which the last example in Q-330:1 has been plagiarized:

"A hysterical 'I cannot see' means 'I do not want to see.' It indicates a repressed impulse to look (and to exhibit). From a punitive standpoint it says: 'Because you wish to see something forbidden, you shall not see at all' " (Fenichel, 1945:226)

§331. A few methodological points from a later volume may be anticipated at this point.

If the task is to answer the question: How did Freud proceed in order to derive and justify his interpretations; then, either an *extensional* or an *intensional* approach may be applied. One cannot say that any of these is per se less legitimate than the other. However, it is much easier to apply the *intentional* approach in an improper way, so that numerous extremely keen philosophers and methodologists will not notice the error.

After having applied the intensional method, we may come up with an answer like the following. Freud first collected all data of each case. Second, he compared all of them with each other. Third, he found the unique interpretation that is the only one which will explain each and all the data.

The extensional method will instead consist of the following steps. First, many *concrete instances* must be collected and listed, of interpretations together with the observations that are claimed to prove them. We may indeed talk of *the method of juxtaposition*. Second, we may ask what is the common denominator of all the concrete instances.

The intensional method might lead to the following particular answer. Freud discovered that oral eczema is invariably caused by fellatio having been practiced upon the patient during infancy. The extensional method will immediately reveal that Freud merely inferred the infantile events by means of the principle of similarity.

Evidently, no one need see a patient in order to produce this variety of interpretations. And evidently, no amount of analysis of the relation between all data will give the slightest hint as to any psychoanalytic interpretation, unless the principle of similarity be applied.

§332. A partial admission of this fact may be found in *The Psycho-Analytic View of Psychogenic Disturbance of Vision* (GW-VIII:94-102/SE-XI:209-218). Freud expresses his contempt of the research on hysterical blindness performed by Charcot, Janet and Binet. I am familiar with Janet's contributions, and shall eventually publish a separate report on Janet's specific and highly sophisticated methodology. Here, I may vouch for the high quality of his research, together with one feature markedly distinguishing the latter from Freud's: Janet's patients were really hysterics, and not just individuals with all kinds of somatogenic ailments.

Freud then proceeds to reveal the superiority of psychoanalysis. But he merely asserts that blindness is caused by an unconscious act of will. The purpose is to suppress the enjoyment at seeing sexual matters.

All this is presented as thoroughly established facts. And the reader is given the impression that Freud describes results obtained by actual psychoanalytic treatment of a considerable number of blind patients. However, 18 lines from the end of the paper he states:



"These 'neurotic' symptoms are unfortunately little appreciated and understood even today; for they are not directly accessible to psycho-analysis, and other methods of research have left the standpoint of sexuality out of account." (GW-VIII:101/SE-XI:218; Freud's quotation marks in GW have been added) [Q-332:1]

According to the psychoanalytic *core theory*, psychogenic symptoms are caused by the fact that conscious memories or emotions etc. have been banished from the conscious. They will immediately disappear, when the banished phenomena are permitted to re-enter conscious.

In fact, a true interpretation may be distinguished from a false interpretation, by the [alleged] fact that the former and only the former will produce permanent symptom removal.

It is difficult to escape the consequence that psychogenic blindness is eminently suited for psychoanalytic treatment, and that the correct interpretations will indeed cure blindness.

Q-332:1 may be interpreted in two senses. *Either*, Freud has *not* tried psychoanalysis on cases of hysterical blindness. Then he proves that it is not necessary to see a patient in order to produce *conclusively verified* interpretations of the patient's symptoms. *Or else*, Freud has tried psychoanalysis *and failed*. That is, his clinical observations reveal that his theory is false.

§333. We are not yet through with Michael. We may certainly want to know much more about the specific features of and around his symptoms. It may be no trivial question whether Michael's right or left leg was afflicted, since the possibility should be ruled out that a different leg was used during the infantile event.

Moreover, how serious was Michael's affliction? Did he remain in bed? or did he go outside the house, either occasionally or on every day? Did he suffer from permanent or intermittent stiffness? Did he use crutches?

How did he react to the stiffness? With resignation; depression; indifference; anger; obstinate but vain attempts to conquer the handicap; trying to cause as little trouble as possible; putting other people in his service; joy over redemption from a boring school or job; etc.?

Did he have a wife or girl-friend? If so, how did she react?

We may also like to know how often Beatrice and Florence had anxiety attacks, and how strong they were.

§334. I shall now formulate an additional hypothesis. On 31 October 1895 Freud wrote to Fliess:

"My 'bashful' case must be finished by the end of '96. He developed hysteria in his youth and later showed delusions of reference. His almost transparent history ought to clear up a few disputed points for me. Another man (who does not dare to go out in the street because of homicidal tendencies) ought to help me solve another puzzle." (Freud, 1985:148) [Q-334:1]

Two days later, on 2 November, Freud continued:

"Today I am able to add that one of the cases gave me what I expected (sexual shock—



that is, infantile abuse in male hysteria!)” and that at the same time a working through of the disputed material strengthened my confidence in the validity of my psychological constructions.” (Freud, 1985:149) [Q-334:2]

This is the first time Freud attributed infantile seduction to a case of male hysteria. But a crucial circumstance must not go unnoticed. *If Freud told the truth on 21 April 1896, and if he succeeded in producing de-repression of an infantile seduction event on the first Saturday of November, he also observed the much more remarkable phenomenon of symptom removal. But if he did observe such a noticeable phenomena, it is hardly believable that he would forget to mention this result in his letter.*

If the patient was the one who recalled the seduction event after de-repression, it would be incomprehensible why Freud talked about “MY psychological CONSTRUCTIONS”. Note also one more unnatural formulation. What would we think of an individual who, after having watched a solar eclipse for an hour, said: “My observations today has ‘*strengthened my confidence*’ in the hypothesis that there really are solar eclipses”?

The brief quotation of Q-334:2 comprising only 48 words, contains no less than three formulations, each of which is hardly compatible with the suggestion that Freud told the truth in his published seduction papers.

§335. *Could the male patient with homicidal tendencies be Michael?* If so, Freud must have reasoned as follows. 1. Michael’s stiff leg *actually prevented* him from going out. 2. Consequently, his unconscious had produced the stiffness *for the purpose of preventing* his going out. 3. The specific aim was to prevent Michael from performing a forbidden act he wished to commit. 4. The act consisted of killing someone.

It is one of the cardinal principles of psychoanalytic interpretation procedure to take for granted that any effect actually achieved, was also intended. Many readers may nonetheless object that the hypothesis of Michael’s identity with the other patient, is arbitrary. Some of these readers may previously have encountered the following quotation from the paper on hysterical blindness referred to in §332. Fenichel (1945:226), characterizes it as “very enlightening”.

“This relation of an organ with a double claim on it—its relation to the conscious ego and to repressed sexuality—is to be seen more clearly in motor organs than in the eye: as when, for instance, a hand which has tried to carry out an act of sexual aggression, and has become paralysed hysterically, is unable, after that act has been inhibited, to do anything else—as though it were obstinately insisting on carrying out a repressed innervation; or as when the fingers of people who have given up masturbation refuse to learn the delicate movements required for playing the piano or the violin.” (GW-VIII: 100/SE-XI:216f.) [Q-335:1]

Recall from §334 the fact that the so-called results obtained in Freud’s paper on blindness, were not obtained by the clinical study of real patients.

I do not think it makes much difference whether the forbidden act is sexual or aggressive.

A final comment. We have here one more illustration of *the illusion of*

*separation*. In musical performance a few points of the body may be the object of considerable physical strain. Numerous musicians will wear out particular joints. This well-proven etiology was by Freud implied to be non-existent. Instead he mechanically applied *the principle of similarity*. Musicians and others who suffer from lame joints supposedly want to protect themselves from masturbation.

As for the hypothesis of the identity of Michael and the patient who experiences homicidal tendencies, the reader must evaluate for himself the power of the evidence.

§336. We shall now turn to the biography of Florence. Recall that Freud is very prone to "prove" his interpretations by fabricating *temporal* relations.

a. We are told that her anxiety attacks "*preferred*" certain hours of the day. This term is awkward in the present context. It may mean at least three different things. 1. The anxiety attacks occurred almost exclusively at these hours. 2. They were slightly more frequent at these hours than at others. 3. The attacks were evenly spaced over the entire day without any particular hours being predominant; but it is gratuitous to claim that a phenomenon is *slightly* more frequent than it really is.

b. It is suspicious that Freud says nothing about *what hours* were preferred. If the evening was preferred, most readers would guess that darkness had something to do with the anxiety attacks. Cf. *the illusion of separation*.

c. Allegedly, Florence had "numerous" sisters. Since Freud recurrently fabricates, it is by no means impossible that Florence only had one sister. But I shall disregard this circumstance.

However, we are told nothing about the number of sisters, nothing about their age, nothing about their temperament. What if only one of the sisters had a particularly empathetic and consolatory temperament? Possibly, only one sister was not significantly older than the patient. Or possibly, only the preferred sister was much older: she might always have held a mother-like position among all siblings. We cannot be sure that Freud would have told us the truth, if he had known that the patient's mother had died when she was very young.

d. But on Freud's premise: since Florence had numerous sisters, why could a sex act not be performed if the preferred sister was at home; while the presence of any other sister or even of all the other sisters was no obstacle?

e. Why did the man always *ask* whether the preferred sister was at home? Couldn't he use his own eyes?

f. *Whom* did he ask? Why did the person he asked not constitute any obstacle?

§337. The preferred sister could evidently not have been both alive when Florence was 2–4 years old, and significantly younger than Florence. Among the remaining possibilities, I shall first try out the assumption that she was significantly older, and then the idea that both sisters were of more or less the same age.

g. Isn't it strange that the preferred sister was always available when Florence had her attacks? Hadn't she married and moved to another house?

And if there was little age difference:

h. *Where* had the sister gone when she was not at home? If she was at granny's why wasn't Florence there too?

i. Were two very young children left completely unattended by those responsible for them?

j. What kind of a sexual act did the man perform? Freud claims that the seductive acts do *not* consist of sexual intercourse. But not every kind of acts which could lead to a prison sentence, need be very traumatic to a child.

k. If the preferred sister was absent during an attack, how did Florence react?

All these questions do not prove that Freud's account is impossible. A pattern of data *can* be constructed which is internally consistent, and is also consistent with all external circumstances that *must* have pertained. I shall present such a pattern in Q-340:1. However, rather exceptional details are needed in order to make the account internally and externally consistent. Flagrantly, Freud is unaware of the strange nature of his biographic data. He would hardly have produced such an account, if the latter had been based upon authentic observations, or if he had at least believed in his own words.

§338. *It is one of Freud's recurrent techniques to blame the patient for defects for which he himself was unambiguously responsible.* All information really or allegedly supplied by Dora, forms a perfectly coherent and consistent picture. By contrast, Freud's own contributions to the case-study are extremely confused and contradictory. Nonetheless, Freud excused the incoherent nature of his paper by the claim that it is impossible to produce a coherent case-study of a hysteric, because *the patient* will invariably give incoherent and contradictory accounts. (The logical structure of this argument is invalid. A non-contradictory analysis may well be based upon a set of contradictory statements.)

Moreover, we have seen that Freud himself forced upon his patients his own seduction interpretations. Nonetheless he later claimed that he had eventually exposed that *the patients* had merely fabricated.

§339. There is strong reason to expect the objection that Freud was innocent, while Florence was responsible for all the defects in her biography. This is a typical *one-step argument* (cf. §101). And it will collapse as soon as it is spelled out completely. Florence told Freud that her anxiety attacks "preferred" certain hours of the day. It never occurred to Freud—who was a keen clinical observer and who always paid close attention to the most inconspicuous details—that the supplied information was deficient. Nonetheless, Freud told the truth, when he claimed that each and all patients *including Florence* told him hundreds of details, as if they were living through the original event. If the latter took place in the evening, Florence told Freud so, and explicitly stated whether it was dark or dusk. Moreover, Florence told thousands of details connecting the sexual event to her general life. In particular, she told such details as conclusively ruled out the possibility that the sexual event was merely fabricated.

Obviously, the blame cannot be shifted to Florence, and Freud's account cannot be saved. Even if we had no other information than that presented in §§336f., we would be entitled to conclude that Freud was not telling the truth. Florence

never claimed to have been seduced during childhood. Freud himself had fabricated the seduction event and put it into Florence's mouth.

§340. I shall now construct a fictive biography about Florence. Eventually, I shall discuss a number of things that may be learned from the latter.

Florence's parents were born in 1829 and 1834. They had four daughters born in 1853, 1854, 1857 and 1859. From 1860 to 1872 her father was in prison because of a "crime" of conscience. In December 1873 two heterozygotic twin sisters were born, Florence and Zoe. There were no sons in the family. The older sisters eventually married and moved to other houses. The last one married when Florence and Zoe were 10 years old.

During 1877 the mother's brother's son, then 17 years old, frequently visited the family in the afternoon. He usually arrived about 3 o'clock, after having finished school. He had a kind and helpful temper. He loved children also for non-sexual reasons, and he was spontaneously loved by them. Hence, he was usually left alone with the children. He and Florence and/or Zoe would usually be the only persons present at the upper floor. And the old stairs would give conspicuous testimony, whenever anyone went upstairs.

This teenager had a peculiar habit. When alone with Florence he would smear his erect penis with honey and have Florence lick it away. It is not known whether he did the same thing when he was alone with Zoe, although we may guess that he did. He never used any force, nor did he use any threat except never to do it again if Florence told anyone, including Zoe. Since the children were never given sweets in their puritan home, the threat worked. (The reality of the seduction events was directly verified by Freud by means of hypnotic age regression.)

At an unusually early age Zoe showed a highly altruistic attitude. Moreover, she was only 12 when she decided to become a nurse. Later, she actually obtained a certificate from a school for nurses. At the age of 24 she took the veil and, by her own free choice, came to work at a colony for leprosy patients in Madagascar. It is known that she had an exceptionally high capacity for encouraging and consoling other people.

One day when Florence was 16 years old, there was a solar eclipse. It started at 3 o'clock. By coincidence, Florence was witnessing a fire when the eclipse started. A home for aged people was burning. Half the inmates were burned alive, and Florence saw many of them hanging from the windows screaming.

From that day Florence had recurrent anxiety attacks for 13 years. She belongs to those patients for whom Freud's theories could not be confirmed by means of therapeutic success.

Most of her anxiety attacks started at 3 o'clock in the afternoon. For all exceptions to this rule, there were palpable explanations. Two attacks started at the beginning of solar eclipses. One started when a lover of practical jokes had changed the clock so that it struck three at 11 o'clock a.m. The attack was no less violent, although Florence was aware of the fact that it was not yet noon. All other attacks had obvious relations to fire: Florence might have read in the newspaper about a fire.

Whenever she was overwhelmed by anxiety, she wanted Zoe to be present. When Zoe went to the school for nurses, Florence would sit or walk in a nearby park, each day from 3 to 4 o'clock during the whole year, even in winter, although she would seldom suffer more than 3 attacks during one month. When she had an attack, she would sometimes be permitted to see Zoe. But even if she was not, she felt a small consolation from being able to see the building that Zoe was in. [Q-340:1]

§341. Note first of all that it is fairly obvious *what* details must be deleted or modified in order to protect Florence's anonymity. It is directly seen that such changes will leave intact all features of any psychiatric significance. By contrast it is Freud's habit *not* to delete facts which will expose the patient's identity or his

most intimate private life. We have already seen illustrative examples in ch. 6.

Second, the presence of "convincing details" does not guarantee that an account is not fictive or faked. But *the absence* of such details is hardly compatible with the suggestion that an account is authentic.

Third, numerous behavioural scientists who are not particularly favourable to psychoanalysis, have no idea as to *how* satisfactory clinical evidence would look. They are incapable of seeing *what* kinds of data are missing in Freud's writings. They may enormously underrate the difficulty of producing any set of clinical observations which would support psychoanalytic interpretations. They may overrate the likelihood that Freud had any real evidence.

Fourth, whenever the academic community states that Freud's descriptions of Florence and the other 17 patients are brilliant, empirically careful, highly informative, and most convincing, the academic community indirectly encourages the psychoanalysts' endeavour.

§342. Fifth, my fictive biography is in every respect compatible with Freud's biography of Florence. Not one single datum in the latter will provide any indication as to whether the former is true or false. One could not easily imagine any circumstance which would provide more powerful evidence of the vacuity of Freud's account.

Psychoanalysts might raise a few objections. First, if we had not *known* about the fire, the causal connection between the anxiety attacks and the infantile event would have been much more probable. Second, if Florence had experienced any event such as the fire, Freud would have discovered this fact, and he would have communicated it to the reader.

But we have already seen in §211 that Freud completely overlooked the fact which was most probably causally responsible for Dora's birthday mood; and we shall encounter numerous analogous examples elsewhere—not least in connection with *The Illusion of Separation*, a principle to be discussed in a later volume.

And in a later volume we shall see how Freud proved Dora's homosexuality by concealing from the reader 10 1/2 out of the 12 pieces of information which were available to him. Freud was aware of the fact that the concealed information is hardly compatible with the suggestion that Dora was homosexual. Or to put it in another way: he was aware of the fact that Dora was not homosexual. He disseminated the opposite assertion together with numerous clues which would guarantee that she would be recognized by almost all neighbours, servants, fellow students, distant relatives, and so on. But first and foremost: Freud realized that few readers would believe his claim about Dora's homosexuality, if they were told all the facts.

§343. One more objection of the type frequently advanced by psychoanalysts: Freud deleted the information because of space considerations.—The best answer to this idea was given in §250. I shall however illustrate how little space is needed for describing those details which would not be missing, if Freud had made genuine observations. The following account is taken from Michel Kpomassie: *L'africain du Groenland*:



"Aqqaluk, 11 years old, lays down on hands and feet in the sleeping room in front of his little brother Agangut, 2 years old, who is standing in the bed, dressed only in a shirt, takes his little penis in his mouth and starts to suck it. Everyone laughs when the child is shivering and gets an erection. Aqqaluk repeats this play twice or thrice a day without being reproached by anyone." (Kpomassie, 1984:179, transl. by MS from the Swedish translation) [Q-343:1]

A comparable instance in Ingmar Bergman's (1987:128f.) autobiography comprises 191 words.

(Obviously, if Agangut had developed a neurosis 20 years later, many more words would be needed to prove that the described instance of "sexual seduction" was etiologically related to the neurosis.)

§344. It is time to ask the following questions: What kinds of sex acts did the children experience? How old were they? Were the children's partners adults or other children?

An important preamble. I would like to introduce into English an excellent Danish and Swedish expression, which will capture the essential point more aptly than the expression "a hidden reservation". In 1520 the Danish king Kristian II decapitated some 80 prominent Swedish nobles; the event is called the Stockholman Massacre. Most of the executed persons had signed a certain document directed against the king. The life of Bishop Brask was however spared, because he could prove that *under his seal* he had retracted his signature. Since then, the expression "*a Brask-note*" is colloquial in Danish and Swedish.

We shall find a wealth of Brask-notes in *Gesammelte Werke*.

§345. According to the *second* seduction paper: "In two cases the series started in the little creature's second year (?) [Freud's question mark]; the commonest age in my observations is the fourth or fifth year (GW-I:418/SE-III:152). Freud uses the phrase "the fourth year" in the incorrect sense of the interval between the fourth and fifth birthdays. In the *third* paper we are told that the seduction happened "during the third, fourth, *even* during the second year of life" (GW-I:449/SE-III:212, italics added). The discrepancy between the age indications does not suggest a conscientious researcher who had studied the matter carefully. But this is the least defect. *The order* of the list, as well as the word "even", would be most awkward if Freud had really gathered empirical evidence as to the age for all 18 patients.

Moreover, it would be a superhuman task to certify that a certain recollection refers to an event which took place *after* the two-year-birthday but definitely not *before*; and likewise *before* the five-year-birthday but definitely not *after*. And despite this heroic achievement of the patients' memory, we are not told anything as to whether the event had occurred early or late during this interval.

Note, *it is much easier to fabricate a vague and abstract untruth, than a concrete and detailed one*. It is gratuitous to assert that previous adult seduction was disclosed for all patients; that repressed infantile causal events were uncovered for all; that the original seduction acts took place at the age of 2-4; and that most patients were treated for 100 hours or more. *What Freud does not manage, is to*



*fabricate those numerous small details which would definitely not have been missing, if he had discovered the phenomena he claims to have established—or if he had at least believed in his own words.*

§346. In §115 I discussed certain features of Bonaparte (1945). According to her, an adult couple had practiced sexual intercourse and fellatio in the presence of a young girl. They had allegedly done this in full daylight until the girl was two; but thereafter only in the dark. And the proof is that all children learn to speak at exactly this age, so that the child might otherwise have given the couple away. By implication, all people are so rational, that they will never take the chance if the sap is rising while the child is having a nap.

But if it is that easy to disclose the maximal age, it is strange that Freud stated that his seducers *started* their activity at the age (of the children) at which *anyone* would have *stopped* taking the risk.

Besides, it is somewhat surprising that none of Freud's patients ever told their parents about any of their experiences—not even if the act consisted of rape by a stranger.

§347. In the second seduction paper Freud explicitly talks of “intercourse” (“une liaison infantile”) in relation to acts performed by a girl of 2–5 and a boy who was only “a *little* older” (GW–I:417/SE–III:152). What could he possibly mean?

I shall now supply a magnificent illustration of a Brask-note. Freud makes a strong assertion as to the nature of the sexual events, using italics of the words I have given in bold type:

“Es handelt sich bei ihnen nicht mehr um die Erweckung des sexuellen Themas durch einen beliebigen Sinneseindruck, sondern um sexuelle Erfahrungen am eigenen Leib, um **geschlechtlichen Verkehr** (im weiteren Sinne).” (GW–I:439/SE–III:203, italics deleted, bold type and small print added) [Q-347:1]

“It is now no longer a question of sexual topics having been aroused by some sense impression or other, but of sexual experiences affecting the subject's own body—**sexual intercourse** (in the wider sense).” (GW–I:439/SE–III:203, italics deleted, bold type and small print added) [Q-347:2]

In an analogous way Freud talks of “koitus-**ähnlichen** Handlungen, “coitus-like acts” (GW–I:443/SE–III:206).

This expression is clearly applicable to both pure child sex and seduction involving an adult. In other words, sexual intercourse was *not* involved.

Hence, the expression “Szenen à trois” (“group sex”) (GW–I:442f./SE–III:206) is manifestly misplaced.

§348. Were the partners adults or other children? It seems rather clear that 7 females belonging to the February sample—that is, 58%—had never experienced anything more than pure child sex. We may take Freud's fabricated statements at face value and explicate the proportions they entail. Thereby, we shall arrive at fictitious results. Nonetheless, I shall generously give Freud a chance. 1–

2 male newcomers to the April sample had likewise never been seduced by an adult; and it is possible that Christina had not either. Hence, 8–10 out of 18, or 44–55% had solely participated in pure child sex. (Cf. also §369.)

A passing remark. The reader may decide for himself whether it is likely that Aqqualuk (cf. Q-339:1) had previously been seduced by an adult, and had merely repeated the same kind of act which the adult had practiced upon him.

But it seems perplexing that children's sexual, semi-sexual or pseudo-sexual play should have such a powerful pathogenic effect.

§349. The last few paragraphs are not without relevance for the biographies of Desirée and Elsa. They had participated in group sex together with the same "male person" ("mit der nämlichen männlichen Person") (GW-I:442/SE-III:206). Since the German language does not distinguish between "male" and "manly", the reference to a "man-like" person or an individual of male sex, without any indication as to whether Freud had a child or an adult in mind, is curious. I believe Freud was thinking of a child, for two reasons. My first reason may not be strong. If Freud had an adult in mind, I cannot imagine any motive which might have lead him to choose such an awkward expression. But if *anyone* talks of group sex and is thinking of children, the oldest one of which is at most a little older than 4, he has good reason for using an expression which is compatible with this idea, but does not actually assert the latter.—My second reason will be stated in a moment.

§350. We have already encountered waterproof evidence of the proposition that all the infantile events were constructed by Freud; and that Freud was aware of not telling the truth when he put these accounts in his patients' mouths.

Allegedly, the memory were "extracted from [the patients] piece by piece", "under the most energetic pressure of the analytic procedure, and against an enormous resistance" (GW-I:418/SE-III:153). Since the "scenes à trois" were not extracted at all from Desirée and Elsa, Freud's words are deliberately false. However, they are not purely fictive: they constitute a distorted version of the true state of things. From the case of G.de B. we can easily see that: while *the interpretation* was presented by Freud in one stroke, *the patient's assent to the interpretation was indeed extracted "under the most energetic pressure of the analytic procedure and against an enormous resistance"*. From the case-study of Dora we can see that Freud never succeeded in extracting Dora's assent to any interpretation (there are two faked exceptions, as I shall show elsewhere). But he certainly did his best—and might well have succeeded, if she had had a less independent personality.

§351. From some of Freud's postulations which cannot be doubted, we are entitled to infer that the group sex events were similar to Desirée's and Elsa's symptoms, or to certain circumstances closely related to the symptoms.

I shall now present a hypothesis; and I shall make no attempt at concealing that the evidence is inconclusive.

First, I think that Desirée and Elsa were really identical with Alice and Beatrice. They might have been sisters, cousins, neighbours, schoolmates etc. Al-

though Freud might well have fabricated the antecedent event, I think that Alice and Beatrice were really sitting at a table together with a boy. They might have been, say, 14–15 years of age. The boy stroked Alice's hand, and pressed his leg against her gown. Beatrice heard him saying (rather than really asking) a question to Alice. I fancy it cannot have been very daring. He might have asked: "Guess what I dreamed about you last night?"

We need not take seriously Freud's statement that the boy's facial impression imparted to Alice the idea that he was doing something forbidden. It is as likely that he was somewhat unsure as to whether Alice would tolerate his advances. And Freud has repeatedly demonstrated his inability to identify human emotions. For instance, the phrase "Kiss my arse!" evidently is used to express contempt. But Freud (GW-VII:207/SE-IX:173) imagines that the phrase expresses defiance. Moreover, his description of the rat man's facial expression when the latter recounted the punishment scene (GW-VII:392/SE-X:166f.) is simply not believable.

§352. What to do about the claim that the antecedent event was "discovered with so much trouble and extracted out of all the mnemonic material" (GW-I:436/SE-III:200)? The formulation might be a kind of "slip of tongue" deriving from carelessness: Freud might be thinking of *other* fabrications, for which he indeed took much trouble to extract Alice's consent (whether or not he was successful).

But a second hypothesis is as likely. The teenager event had no temporal or causal relation to the emergence of Alice's painful sensations in vagina. The event might have preceded the first occurrence of vaginal pain by several years—or vice versa; in the case-study of Dora Freud repeatedly reverts the authentic order of the historical events to "prove" his interpretations.—If the second hypothesis is true, Freud must have fabricated the interpretation that Alice *unconsciously* reproached herself because of the boy's hand stroking—whereafter Freud fabricated an observation from his interpretation.

§353. Nothing hinges upon which one of these alternatives we choose. But the former hypothesis is not entirely coherent. If Alice got ill because of such a petty experience as hand stroking, it is incomprehensible that the facial expression of someone else was needed for her to learn that hand stroking was forbidden behaviour.

§354. But according to my hypothesis: when Freud heard about the teenager event, he reacted like certain testees upon Rorschach inkplot cards. The idea spontaneously occurred to him that precisely these three teenagers had participated in "group sex" when they were 2–4 (and the boy perhaps "a little" older).

Alice's vaginal pain might well have had a purely somatogenic etiology (a pimple or a fissure?). But Freud fabricated that Beatrice had likewise got ill from the group sex acts, and had eventually become Freud's patient. (We have seen waterproof evidence to the effect that the external verification was faked.) I do not know what kind of sexual behaviour Freud thinks the three children performed. But he fabricated that Beatrice had likewise got vaginal pain from the

event. He also felt that his account would look more plausible, if the two females did not have exactly the same symptoms. Anxiety attacks constitute exactly the symptoms which it is most gratuitous to attribute to an individual to which hysteria had already been attributed.

(A number of alternative variant of this hypothesis will be equivalent for our purpose. I shall not waste space upon these.)

It is not very important whether Freud treated Alice for a year or a month, or saw her only once. But the hypothesis entails that not only the observations about some of Freud's "18" patient are faked—some of the very patients may be too. We shall return to this question in the chapter *How Many Patients Did Freud Have?*

§355. Further comments on Christina than those already given may not be called for. Instead, we shall look at Freud's theory of the nature of the seduction event (GW-I:428, 439/SE-III:193f., 203). The latter must satisfy both a quantitative and a qualitative condition in order to produce a hysterical symptom. It must be *sufficiently traumatic*. And it must be *similar* to the symptom.

In 1896 Freud used the term "traumatic" in its normal sense; a layman would be capable of judging whether or not an event is traumatic. Not until a much later time did Freud grant himself licence to attribute *unconscious* traumatic feelings, where there is no conscious or unconscious sign of any conscious or unconscious traumatic experience.

The quantitative rule would be legitimate as *an established empirical generalization* or as *a heuristic tool*. But Freud applies it as *an aprioristic principle*.

All psychiatric experience contradicts both the quantitative and the qualitative conditions. Freud tries to explain away the facts by invoking an additional postulate: if an event which appears to be the cause does not satisfy both principles, then this event is merely an intermediary link in a causal sequence, and behind it another event can be found which does satisfy them and is the real cause.

§356. We may now ask whether these two conditions are satisfied by the causal events dug out by Freud—that is, by Freud's own paradigmatic examples.

Whether the infantile events experienced by Michael, Florence, Desirée and Elsa were sufficiently traumatic, depend upon a number of circumstances about which we are told nothing. Cf. §321.

If Freud had really made numerous observations entailing the quantitative principle, he did not realize the fact that the presenting of some of these observations to the reader is a *sine qua non* of scientific communication.

Furthermore, a number of Freud's statements seem to suggest a *non-traumatic etiology*: the seduction event may be so pleasant to a child, that he or she may develop an appetite for repeating this kind of events.

Digression. Since 1968 and at least 15 years onwards, psychology was overrun by the ideology that quantitative methods, if applied within any behavioural science, will necessarily yield invalid results. This ideology was used to fight experimental psychology.

Most proponents of anti-quantificationism advanced psychoanalysis as an altogether satisfactory alternative.

§357. Is the qualitative condition satisfied by Freud's paradigmatic examples?

It is clearly so for Michael. But in the case of Florence the similarity relation is found at the wrong place. The seduction event is not directly similar to the symptom. Instead, *the non-sexual preamble* of the seduction event is similar to *the situation releasing* the symptomatic attacks, and also similar to the patient's *reaction upon the symptom*.

If Alice and Beatrice were identical with Desirée and Elsa (whether or not Beatrice was neither ill nor a patient of Freud's), the infantile seduction event is similar to *the antecedent situation* which preceded the emergence of the symptom.

In other words, Freud's theory is *falsified* for three patients out of a total of four.

Freud never noticed that in all instances but one, the similarity relation is missing where it is implied by his theory, and found where it will not support the theory. Unsurprisingly, none of his followers have detected this flagrant defect.

§358. According to Freud (GW-I:454/SE-III:216f.), it is only because we know no more than a fraction of the motives behind a hysterical reaction, that the latter may appear exaggerated to us. To indicate the importance of this statement, Freud has given the latter in italics. "In reality, this reaction is proportionate to the exciting stimulus."

At the moment I shall disregard one trivial and one highly important circumstance. On the one hand: still today we cannot measure the strength of the reaction or of the exciting stimulus with such a degree of precision, that it could be empirically established whether or not Freud's claim is true. On the other hand: after having read the third seduction paper, we still know no more than a fraction of the motives of Alice, Beatrice, Florence, and Michael. After Freud's analysis, their reactions seem as exaggerated as ever.

As was pointed out by Scharnberg (1984, §139), exactly the same feature is present in the case-study of the rat man. The patient felt a terrible dread at his compulsive fantasies of rats eating up the buttocks of a human victim. Freud does not permit us to apply the common sense explanation; we are *requested* to apply *the illusion of separation*. But there is gap between the patient's horror and the *undramatic* causal factors claimed to explain the latter. In what sense is it easier to understand the horror, if we know that the rat symbolized a child, and money, and a penis?

This is one variety of objections which have not often been advanced. Although Edvard Westermarck must also be mentioned, Hans-Jürgen Eysenck seems to be the critic who has most strongly emphasized Freud's lack of knowledge of human nature.

§359. Both normal and pathological reactions will often deviate enormously from the proportionality axiom. Casual observation, careful clinical observation, and controlled experimentation, unanimously agree in this respect. There is no room for the hypothesis that the discrepancy merely derives from our ignorance of some of the causal factors involved.

§360. Firstly, Freud supports the proportionality doctrine by a *persuasive* argument (GW-I:455/SE-III:217). When a married couple react violently to a trifle, we are justified in concluding that the reaction is to some extent caused by occurrences in the previous interaction of the couple.

Now, this conclusion is *not* invariably justified: physical illness, previous social interaction with *other* people (inter alia, having experienced torture in a prison), and numerous other circumstances, may often lead to “exaggerated” reactions.

Secondly, our inference about what is likely to be the case, is derived from an *empirical generalization*: we have in the past observed numerous confirmatory instances. By contrast, Freud applies his doctrine as an *apriori principle*.

Thirdly and most importantly: Freud applies his doctrine to Alice. Her exaggerated reaction to the boy’s stroking her hand, is claimed to be the result of her previous experiences. I will grant this. But Freud takes the position that: granting that much is equivalent with granting that Alice’s reaction depended upon previous experiences *of the same nature*—in other words, upon experiences of *sexual behaviour performed together with a male*.

Here, Freud reveals his ignorance of human nature. He also applies *the illusion of separation*, and spontaneously postulates the non-existence of such causal factors as a puritan upbringing, hostile to sexuality.



## Further Comment Upon Missing or Vague Information

*"Is there any point to which you would wish to draw my attention?"*

*"To the curious incident of the dog in the night-time."*

*"The dog did nothing in the night-time."*

*"That was the curious incident," remarked Sherlock Holmes.*

Conan Doyle

§361. As far as I have been able to find out, the first psychoanalyst ever trained by Freud was *Felix Gattel*. In 1898 he published 100 patient biographies for the purpose of testing Freud's theories of anxiety neurosis, neurasthenia, and hysteria. This writing of his will be extensively presented and analyzed in vol. II. Inter alia, I shall quote in English translation the total text of all Gattel's 18 biographies of hysterics, together with his day-to-day case-notes of a psychoanalytic treatment performed in 1897—probably the oldest document of this kind in existence.

There is strong reason for anticipating a few details now. Years before I could possibly have learned about Gattel's paper, I have repeatedly asserted that *Freud's secret data were of exactly the same nature as his published data*, viz. 1. exceedingly few in number, 2. exceedingly trivial, 3. available to any layman outside of the psychoanalytic situation, 4. totally wanting in evidential power, 5. containing no instance of de-repression, 6. containing no instance of recovery; etc. I deduced all these features by means of textual analysis of Freud's published writing.

Now it turns out that Gattel's biographies are of exactly the same nature. But the most crucial point is *Freud's extraordinarily positive evaluation of the evidential power of Gattel's contribution*.

§362. Gattel's cases contain the following categories: case no./name of patient/profession/age/duration of symptoms/heredity and previous sickness/ subjective complaints/somatic findings/sexual behaviour/diagnosis. In general, we need not here bother about much more than the subjective complaints (= the symptoms) and the sexual behaviour. I shall quote these two categories *in toto* for four patients.

*Case no. 12, male, 18 years of age.*

*Subjective complaints:* Abdominal pain, headache, fatigue.

*Sexual behaviour:* Masturbation.

*Case no. 3, male, 19 years of age.*

*Subjective complaints:* Fatigue, abdominal pain.

*Sexual behaviour:* Masturbation since 4 years.

*Case no. 15, female, 33 years of age.*

*Subjective complaints:* Headache, abdominal pain, dizziness, mild attacks of anxiety.

*Sexual behaviour:* No satisfaction because of fatness of husband. Now and then he masturbates on her.

(Gattel, 1898; transl. and typographical display by MS) [Q-362:1]

60. Johanna K., a shop-assistant. Age: 20. *Duration:* 8 years.

*Heredity and previous sickness:* Fall on her head 8 years ago.

*Subjective complaints:* Insomnia, headache, general anxiety.

*Somatic findings:* None.

*Sexual behaviour:* Her sexual development started at an early age. Menstruation already at 13. Likewise, her breasts and genitals started to develop very early. Sexual dreams occurred at an early age; frequently, sexual excitement at the sight of males. Not possible to disclose whether some sexual influence or other took place during her earliest youth.

*Diagnosis:* Hy.? An.

(Gattel, 1898, transl. and typographical display by MS) [Q-362:2]

§363. From *these* few facts, Gattel takes the following statements to be proved. The symptoms of the first three patients were *caused* by masturbation. The symptoms of the last patient were *caused* by sexual abstinence, although it is possible that the fall on her head had made some secondary causal contribution.

As I shall show later, Gattel is strongly motivated to find evidential support of Freud's theories. But he is not prepared to *fabricate* observations. Instead, he collects all kinds of trivial sexual circumstances, to make up for the fact that he hardly found any non-trivial ones. He may repeatedly point out that this or that patient had started to masturbate, to have sexual dreams, to have menstruation, or to have nocturnal seminal emission, at something called an "early" age—although an unbiased reader may wonder whether these patients really had any non-average experiences.

§364. WHEN GATTEL PUBLISHED HIS CASES, FREUD ACCUSED HIM OF PLAGIARIZATION. Freud wrote to Fliess: "It is embarrassing to me to say to him [= Gattel] that he cannot publish these things as his own property." (not in Freud, 1985; quoted after Sulloway, 1979:515).

It must be noted that Freud had nothing to do with the selection of these cases, nor with the collection of the observations. Freud's sole contribution consisted in the fact that he had presented Gattel with his theories that anxiety neurosis is caused by sexual abstinence; that neurasthenia is caused by masturbation; and that hysteria is caused by some kind of sexual experiences during childhood. (It would be interesting to know whether Gattel learned from Freud the view that masturbation by a partner is as harmful as lonely masturbation.)

Freud had nonetheless expected his name to appear on the frontpage as a co-author, and he was very much impressed by the evidential force power of the observations collected by Gattel.

Try to imagine 1. that a prominent atomic physicist at Cern claimed to have

proved by means of the most complex and sophisticated techniques, that the neutrino is not altogether devoid of mass; 2. that an amateur geologist afterwards collected a set of stones from a beach, and published a paper in which he claimed that some stones are smaller than others, but are nonetheless not altogether devoid of physical mass; 3. that the atomic physicist got angry and accused the amateur geologist of having *plagiarized* the new neutrino theory.

And then try to imagine that Freud was in the possession of 18 patient biographies of the same nature as that of Victor, and claimed that *this result* of his had been *plagiarized* by Gattel's just cited four cases.

§365. Sulloway (1979:515) supplies additional crucial information. First, 15.2.1901 Freud wrote to Fliess:

"I have BEGUN collecting my notes, of things told me by neurotics in my consulting room for the purpose of DEMONSTRATING THE CONNECTION BETWEEN SEXUAL LIFE AND NEUROSIS revealed by such NECESSARILY FLEETING OBSERVATIONS, and of adding my own comments. In other words I AM DOING ROUGHLY THE SAME SORT OF THING AS GATTL [Freud's misspelling] DID when he made himself so unpopular in Vienna." (quoted in Sulloway, 1979:515, capitals added) [Q-365:1]

But if Freud told the truth, 1. he had not *begun* this kind of work around 1901, but had *finished* a complete portion of it four years earlier. 2. He was perfectly aware of the fact that such configurations of data as Gattel collected, are altogether devoid of any evidential power. 3. The kind of work he had done had not been of roughly the same sort as Gattel's; rather, the difference had been as conspicuous as that between the just mentioned atomic physicist and amateur geologist. 4. His observations in 1896 had by no means been "necessarily fleeting", but altogether clear-cut.

Despite these facts, Freud was so satisfied with the content of Gattel's book, that he invited Gattel to accompany him on a three-week tour to Italy.

Sulloway notes one more fact. Gattel's scientific collaboration took place at a time when Freud, according to the psychoanalytic myth, experienced total intellectual isolation. But Gattel was a psychiatrist from Berlin, who had taken the trouble of going to Vienna for the purpose of studying Freud's theory.

§366. In situations involving no risk, numerous commentators have asserted that Freud's third seduction paper contains strong and legitimate (although no necessarily conclusive) support for the seduction theory. But I doubt very much that any psychoanalyst would dare to suggest, in front of a competent critic and an unbiased auditory, that the 7 patient biographies in §243 supply legitimate proof of the contention that most of the symptoms of Alice, Beatrice, Christina, Desirée, Elsa, Florence and Michael, were caused by infantile seduction. In the latter situation, proponents of psychoanalysts will usually revert to the standard device of postulating that Freud was in the possession of a wealth of secret observations of genuine evidential power. The point I want to emphasize is that even the proponents do not dare deny the extremely poor quality of the evidence presented by Freud.

§367. Taking this admission as our point of departure, I shall once more ask

the reader to perform a brief thought experiment. Let the assumptions be as follows. 1. The reader has just produced the third seduction paper. 2. In his desk drawer he has the case-notes of 18 patients; and for each patient the evidence is at least as comprehensive and powerful as the biography of Victor, cf. §§376ff. 4. Nonetheless, the reader has, managed to omit each and all clinical observations stated in the case-notes, except the few trivial data of the 7 patient biographies in §243. 5. After having published the paper, the claims stated in it are met with strong skepticism.

At this stage I shall give the reader two options:

*Option A:* to publish a fresh paper in which the reader includes some of the genuine evidence stated in the case-notes, inter alia at least one complete patient biography.

*Option B:* to accuse the academic community of having rejected the seduction theory solely because of hostility and a prejudiced attitude, which no amount of logical or factual evidence would have any chance of overcoming.

The reader's tasks will be: a. to make up his mind as to which option he would choose; b. to justify why he chose this option, as well as why he rejected the alternative; c. to give a determinate probability to the possibility that he might have chosen the alternative option; d. to try to imagine what causal factors might have lead anyone to accept the option the reader rejected.

As we know, Freud chose the second option. But is it possible that he could easily have produced a fresh paper of genuinely evidential power, while the idea never occurred to him to do so?

§368. Let us now try to distribute the data in the third seduction paper over a Karnaugh-like matrix. We are told that the pathogenic event may involve only other children (C); it may be a one-time event involving an adult stranger (S); or it may be a lasting relation with a familiar adult (F). On GW-I:444f./SE-III:208 Freud does not distinguish a rape-like (R) one-time event with a stranger. But when the matrix will be constructed, I shall distinguish between S and R.—Freud overlooked the possibility of a one-time event involving a familiar adult.

*Here*, I shall include R among S, so that we have a total of three types. According to Freud, *some* patients [how many?] had experienced only one type [which type? the same type or different types for different children?]; *some* patients [how many?] had experienced *two* types [which two types? the same set or different sets of types for different children?]; and *some* patients [how many?] had experienced all *three* types.

*Most* of the patients allegedly had experienced *either two or three* types. Nonetheless, each and all patient biographies contain no more than one single type.

§369. As I showed in §348: if the content of the second seduction paper is taken at face value, 8–10 out of the 18 patients had solely experienced pure child sex. Among the remaining 8–10 patients, the testimony of common sense would not in the first place suggest that not a single one had experienced S but not F; or F but not S. Neither does Freud's own text give such an impression to me. I shall however generously interpret the text as though each and all remaining 8–10 patients had been the victims of both S and F.

Let us now distribute these numbers in *Table 369:1*, for all possible values of C; and for each of these, for one maximally equal and one maximally unequal distribution of the remaining two variables.

Table 369:1.

	number of types	maximally equal distribution for two and three types			maximally unequal distribution for two and three types		
Number of children who had experienced indicated number of types of sexual behaviour	one	8	9	10	8	9	10
	two	5	4	4	8	7	6
	three	5	5	4	2	2	2

It is seen at a glance that: for four out of these six distributions, there is no room for the claim that "most" children had experienced two or three types. The same thing is true of 11 out of the total number of 18 possible distributions.

And if only one single child had experienced only S, or only F, the claim would be falsified for each and all distributions.

If '8+5+5' or '8+8+2' (or any of the five relevant alternatives) were observed, it would be *formally correct* to say that the sum of the last two numbers is greater than the first number alone. But this is not a conclusion that would easily suggest itself to a competent researcher, nor to a layman. It is a vastly more probable explanation that Freud had no feeling of the strange nature of his numerical postulations, precisely because he was fabricating.

§370. The reader should study his own reactions to Freud's account of the different varieties of seduction events, and their relative frequency. I think such detailed claims will inspire confidence in most readers: it is as if Freud knows what he is talking about. Most readers will not notice the vagueness of the information.

Moreover, I think Freud anticipated this reaction. But I do not think it occurred to him that only extremely implausible conditions could make his claims non-contradictory.

§371. In the present paragraph I shall again distinguish between rape-like and non-violent seduction by a stranger. Hence, we shall have four types, R, S, F and C. I shall display or comment upon only the information of the *third* seduction paper.

In the entire paper there is not one single word as to whether Alice or Beatrice or Desirée or Elsa had experienced *any* of these types. I think Desirée and Elsa experienced C, but this is an insufficiently supported inference.

Christina experienced C. Michael and Florence experienced F. We are not told whether any of these three individuals might have experienced any additional type.

Strictly speaking, there may be intermediary phenomena between S and F, e.g. if the ordinary child nurse is substituted by a deputy for a week.

§372. Now to the Karnaugh-like matrix I promised: *Table 372:1*.

Male patients are found in the odd-numbered upper left cells, and females in the even-numbered lower right cells. Any female patient who had experienced a rape-like attack, no non-violent seduction by a stranger, a seduction event with a familiar adult, and no pure child sex, would be found in cell no. 28. Individuals who belong to cell no. 21 and 22 could not belong to Freud's April sample.

If no more than the sex of a patient is known, he or she might belong to any of 15 different cells. On the basis of the totality of information presented in Freud's paper, Alice and Beatrice may still belong to the same 15 cells. Desirée and Elsa may belong to 12 cells. Christina, Florence and Michael may belong to 8 cells.

Strictly speaking, if we do not invoke certain formulations of the *second* seduction paper, Christina's brother might be 20 years older than her—hence, she may really belong to 15 different cells.

(The reader may find all these cells for himself.)

In order not to make the matrix opaque, I have left out one important variable: whether the patient had not merely *participated* in pure child sex, but had *initiated* this variant. Anyway, it is impossible to learn from Freud's paper whether any of the above mentioned 7 patients had during childhood seduced another child.

§373. Let us try to combine the variety of classes with the number of individuals. (When we are talking of classes, the plus sign is of course used in the Boolean sense.)

At least 2 females had experienced S. At least 1 boy had experienced S. At least 2 patients had experienced F. At least 2 patients had experienced C. For 2 males  $\bar{C}$  is true. The male part of  $C\bar{F}\bar{S}$  contains 1–2 individuals. The male part of  $CF + CS$  contains 2–3 individuals.  $C\bar{F}\bar{S}$  contains at least 2.  $\bar{C}\bar{F}\bar{S}$  contains at least 2.  $C\bar{F}\bar{S}$  contains at least 2.  $\bar{C}\bar{F}\bar{S} + C\bar{F}\bar{S} + CFS + C\bar{F}S$  contain at least 2.  $CFS + \bar{C}\bar{F}\bar{S} + CFS + C\bar{F}S$  contain at least 10 patients.

Perhaps a few more equally vacuous statements might be extracted from the third seduction paper. The reader may, if he likes, try as a passtime to distribute this information in the matrix. He will have to come up with equivalent statements like the following: cell no. 1 + 2 + 3 + 4 + 5 + 6 + 7 + 8 + 9 + 10 + 15 + 16 + 25 + 26 + 31 + 32 contain together at least 2 individuals, and any of them may be empty.

This is the true character of a paper which has for generations been praised because of its clarity and high informational value.

Table 372:1.

	RS	R $\bar{S}$	$\bar{R}S$	$\bar{R}\bar{S}$
CF	1 2	3 4	5 6	7 8
CF	9 10	11 12	13 14	15 16
CF	17 18	19 20		23 24
$\bar{C}\bar{F}$	25 26	27 28	29 30	31 32



## The Table of Superlatives and the Biography of Victor

*"Have you brought any letters—any documents—any proof that you are trustworthy and truthful?"*

*"Of a surety, no; and wherefore should I? Have I not a tongue, and cannot I say all that myself?"*

Mark Twain

§374. Because of the extent of my analysis of the seduction papers, there is no room for all subanalyses in one volume. However, I want two subsections to be immediately available to any reader who has the present volume in his hands: the table of superlatives, which really belongs in the book on Freud's claims about the conclusive evidential power of his observations (and which will also be resumed in the volume on Freud's persuasive techniques). And the biography constructed by myself, of the fictive patient Victor, which naturally belongs in the chapters on the method of the jigsaw puzzle. But since we shall scrutinize in the next chapter the pseudo-proofs Freud presents, it may be important for the reader to know how good evidence would look, thereby having a standard of comparison.

Those psychoanalysts who have during nearly a century ardently asserted that they themselves have observed conclusive evidence in their own consultation rooms, would probably be highly surprised to learn that it is not in principle impossible that such evidence could really exist.

§375. In *Table 375:1* I have juxtaposed many (but not all of) the superlatives used in the third seduction paper as a substitute for the presentation of any evidence. It is an effective strategy that: if one is totally devoid of any evidence whatever, one should claim to be in the possession of an immense wealth of evidence of absolutely conclusive power.

Among these superlatives the exact function of the second, and of only the second, may not be altogether clear. Freud first states that hysteria has a sexual etiology; and, as we have seen, he means that hysteria is caused by infantile seduction. But then he adds the following odd assertion. If THE READER will submit this claim to the strictest examination, THEN IT WILL TURN OUT that the claim may be substituted with (is equivalent with?) the claim that FREUD has proved this etiology to hold water in 18 cases, and has, moreover, verified the etiology by therapeutic success for some of these cases.

Several explanations of this unnatural formulation might be invented. I shall not discuss these. But I think no legitimate objection may be raised to my inclusion of this instance in *Table 375:1*.

Table 375:1.

Some of Freud's claims about the power of the proofs of the reality of the infantile seduction events.

<i>die müheseligsten Detailuntersuchungen</i> (GW-I:435)	the most laborious and detailed investigations (SE-III:199)
<i>der strengsten Prüfung unterziehen</i> (GW-I:435)	submit ... to the strictest examination (SE-III:199)
<i>durch den therapeutischen Erfolg bekräftigen konnte</i> (GW-I:435)	I have been able ... to confirm it by therapeutic success (SE-III:199)
<i>mein aus den [Psycho-]Analysen gewonnenes tatsächliches Material</i> (GW-I:439)	the actual material [which] I have obtained from my [psycho-]analyses (SE-III:203)
<i>die Bedenken gegen die Ächtheit der infantilen Sexualszenen aber kann man bereits heute durch mehr als ein Argument entkräften</i> (GW-I:440)	Doubts about the genuineness of the infantile sexual scenes can, however, be deprived of their force here and now by more than one argument (SE-III:204)
<i>das Benehmen der Kranken [ist] nach allen Richtungen hin unvereinbar mit der Annahme, die Szenen seinen etwas anderes als ... Realität</i> (GW-I:440)	the behaviour of patients ... is in every respect incompatible with the assumption that the scenes are anything else than a reality (SE-III:204)
<i>absolut beweiskräftig</i> (GW-I:441)	provide conclusive proof (SE-III:204)
<i>eine ganze Reihe anderer Bürgschaften für die Realität der infantilen Sexualszenen</i> (GW-I:441)	a whole number of other things that vouch for the reality of infantile sexual scenes (SE-III:205)
<i>ein anderer und mächtigerer Beweis</i> (GW-I:441)	another and stronger proof (SE-III:205)
<i>eine absolute Sicherheit</i> (GW-I:441)	an absolute certainty (SE-III:205)
<i>unabweisbare</i> (GW-I:422)	impossible to reject (SE-III:205)
<i>einen wirklich unantastbaren Beweis</i> (GW-I:422)	one other proof, and a really unassailable one (SE-III:206)
<i>der Ihnen aus einer vollständig mitgeteilten Krankengeschichte sonnenklar einleuchten würde</i> (GW-I:446)	evidence which, if you were given the complete history of a case, would be as clear as daylight to you (SE-III:210)
<i>den zahlreichen Beobachtungen</i> (GW-I:449)	the numerous observations (SE-III:212)
<i>gab sich ... mit überraschender Regelmäßigkeit ... zu erkennen</i> (GW-I:451)	were also shown ... (and with surprising regularity) to be (SE-III:214)
<i>erklärte sich müheolos</i> (GW-I:451)	were explained without the least difficulty (SE-III:214)
<i>ich darf Sie bitten, dieselben [= meine Resultate] nicht für die Frucht wohlfeiler Spekulation zu halten. Sie ruhen auf müheseliger Einzelforschung der Kranken, die bei den meisten Fällen hundert Arbeitsstunden und darüber verweilt hat</i> (GW-I:458)	I must ask you not to regard them [= my results] as the fruit of idle speculation. They are based on a laborious individual examination of patients which has in most cases taken up a hundred or more hours of work (SE-III:220)

*man kann den Ergebnissen ... nicht gut widersprechen, wenn man die Methode beiseite lässt ... Es wäre ähnlich, als wollte man die Funde der histologischen Technik mit der Berufung auf die makroskopische Untersuchung widerlegen (GW-I:458)*

one cannot properly deny the findings so long as one puts it [= the method] aside ... To do so would be like trying to refute the findings of histological technique by relying upon macroscopic examination (SE-III:220)

§376. I shall now present the biography of Victor. An attentive reader will realize that each detail is carefully worked out, on the basis of concise considerations.

Victor's parents died in an accident when he was 3 years old. After their death, he was for less than a year reared by his paternal grandmother's sister, who had never married. I shall call her his first step-mother. It is known that she had had the same maid for more than 5 years when Victor arrived. But after that date she dismissed a whole series of maids at a hurried pace, though no one can any longer recall how many. She finally decided to manage the household without a maid. It was generally believed in the family that this decision of hers was the reason why she died a few months after the dismissal of her last maid.

More than 25 years after her death her diary was discovered. Since she was always considered a little bit crazy by the family, her accounts cannot be taken at face value, in the absence of additional evidence. Anyway, she described her reasons for dismissing a total of 8 maids. She had caught all of them in flagrante delictu. They had used 8 different parts of Victor's body as vaginal masturbation instruments. The first act happened already the day after Victor's arrival. At that time she was taken by surprise. But afterwards she kept eyes and ears open, and she is convinced that nothing happened which she did not detect.

The following is the order of the parts of the body being used, and the length of the intervening intervals:

Right foot—43 days—left hand—26 days—left knee—79 days—right knee—37 days—left foot—59 days—right elbow—1 day—right hand—15 days—left elbow.

After the death of Victor's first step-mother and until adult age, he was reared by his maternal uncle's widow who had re-married. At adult age he had no recollections from the home of his first step-mother.

§377. One day when he was cycling at the age of 19, he tumbled. He was not hurt, apart from a few trifling exterior wounds. However, his trousers were split and his sex organs became completely exhibited. A group of 5–10 teenage girls waiting for a bus saw everything and laughed at him. Luckily, he had a coat in his bag, so that he could go home on the bicycle without exhibiting anything. When cycling he felt no unusual sensations in his joints or muscles.

The following night he dreamed that 8 naked girls were masturbating on 8 different parts of his bicycle. Two used the left and right pedals, two used the left

and right sides of the hub of the pedal, two used the left and right ends of the handlebars, and two used the left and right "knee" of the handlebars.

The next morning Victor's right foot was stiff. He told the family doctor about the laughing girls and the dream, but the latter said that such things are quite irrelevant. Many years later it turned out that the family doctor had written down the dream and the event with the girls as an example of superstition, having added a marginal comment: "Another black cat passing just before the patient got ill".

Victor did not have any unusual experiences during the next year. But 43 days after the bicycle accident his left hand became stiff. Eventually, a total of 8 parts of the body became stiff, one after the other, in exactly the same order and with exactly the same duration of the intervening intervals, as in the list above.

All the 8 members and parts remained in the same condition until Victor, at the age of 27, visited a psychotherapist.

§378. This psychotherapist held the view that the cause of psychopathological symptoms will invariably be found in the emotional life. Very often, ungratified sexual feelings will play an important role in the genesis of mental illness. And the genesis can always be traced back to earliest childhood. Hence, the therapist started to burrow for emotional, sexual, infantile, and unconscious causes.

Victor's second step-mother had carefully written down the dates of the first appearance of each symptom. Hence, the agreement with the order in his first step-mother's diary could later be ascertained. But at the time of the therapy, the idea that her notations might be important to the therapist, occurred to no one. Victor himself had confused ideas about the order. In fact, he recounted 3 different orders to the psychotherapist at various times, and all of them happened to be false. He had completely forgotten the bicycle dream.

§379. However, the psychotherapist continually pressed for childhood recollections. And now several remarkable things happened. After 11 months of therapy and during the 137th consultation, Victor recalled how his left elbow had been used. He recalled the name of the maid and the colour of the wall paper in the room. During the act the couple were hiding under the bed. Now and then the maid put small pieces of chocolate into his mouth.

The morning after the 137th consultation Victor's left elbow was no longer stiff. For 12 years he suffered no relapse.

After this success, the psychotherapist could not easily avoid having some expectations concerning the other symptoms. But he saw no trustworthy hint as to the order in which they should be attacked. Nonetheless, all 8 symptoms disappeared in exactly the reverse order of their first appearance, while the intervening intervals had exactly the same length but the reverse order. On the day before any of them disappeared, Victor recalled a further seduction event, which was invariably related to the part of the body that would just be about to heal up.

Victor supplied a lot of details about each event, and it later turned out that they were in perfect agreement with the accounts in his first step-mother's diary—e.g.: which acts had taken place in the maid's room, in Victor's own room, in

the attic, in the coal-cellar etc.; the names of all maids; which maid had spanked him, threatened to spank him, given him chocolate, made him drowsy by alcoholic beverage, promised to buy him a great teddybear, and so on.

§380. When Victor had been cured, the psychotherapist decided to write a book about the case. He asked whether any letters to and from Victor's first step-mother had been preserved, having a faint hope that they might contain some empirical evidence. The search led to the discovery of her diary, in such a place that no one could possibly have touched it since it was packed down shortly after her death.

The notations by the second step-mother and the family doctor were also made available to him. He was struck by the agreement between the symbolism of Victor's dream and theories of sexual symbolism asserted by many psychiatrists since the 1820s. After two years of hard work, his book was completed and published. And in this book he quoted numerous long excerpts from the diary and other documents.

§381. Ten years later an anti-climax occurred. The psychotherapist held a press conference. He stated that he had given second thoughts to the case. He had thereby arrived at the conclusion that Victor had fabricated all the instances of sexual seduction. The latter had only taken place in Victor's fantasy. However, no other circumstance of the case needed be doubted nor modified in any respect.

He was assailed with questions: If this be true, how could Victor recover? (He answered, the fantasies had psychological reality for himself.) Had the first step-mother also fabricated in her diary? (No comment.) What about the coal-cellar and the names of the maids? (No comment.) How could the number of intervening days in three different series at three different times agree so perfectly? (No comment.)

§382. This biography is closely related to the method of the jigsaw puzzle. The latter will be extensively discussed in another volume. A few remarks may however be made at the present place.

First, each observation of the biography of Victor may most easily be rendered in print without any non-trivial information being lost. Likewise, no specific observation situation, nor any specific training in any specific observation method, are needed in order to obtain such data.

Second, there is in this respect no difference whatever between the observations about Victor, and the observations described throughout Freud's *Gesammelte Werke*.

Third, there is an enormous discrepancy between the *pattern* of data presented in, on the one hand, the biography and, on the other hand, *Gesammelte Werke* and the entire psychoanalytic literature. No reader has ever encountered any psychoanalytic account that is even remotely similar to the pattern I have constructed.

It is irrelevant that there may be a certain amplitude as to the meaning of the expression *the method of the jigsaw puzzle*. It is apparent that I have applied something of the kind. And it is directly seen that no psychoanalyst has ever done so. Hence, when Freud claims to have verified the real existence and the causal



significance of the infantile seduction events, by means of the method of the jigsaw puzzle (GW-I:441f./SE-III:205), he was aware of not telling the truth. And so was Fenichel (1945:32), who explicitly states that the psychoanalytic profession as a whole derive and justify interpretations by means of this procedure. And so was Sherwood (1969) who does not use the name of the method, but nonetheless applies persuasive devices to induce the belief that Freud applied this method in the case-study of the rat man.

§383. Fourth and fifth, note that I have in the most demonstrative way neglected the criterion of trustworthy, while I have been very careful to satisfy the condition of independence (cf. §§276ff.).

Sixth, by selecting exactly these data about Victor, and juxtaposing them into a meaningful pattern, I have applied a *single-case method*.

Seventh, *one single case is sufficient for providing conclusive evidence for the truth of Freud's seduction theory*. It is conclusively proved that a. the seduction events really took place, b. these events were causally responsible for the emergence of the hysterical symptoms, and c. de-repression of the seduction events was causally responsible for the disappearance of the symptoms.

§384. Eighth, if such a configuration as I have constructed were ever encountered in the real world, we would have no choice but to conclude that Freud's seduction theory is true of at least one individual.

Unique occurrences do happen in the empirical world. But the configuration at hand is not of such a nature that we might reasonably expect it to be truly unique. Hence, we might justly *expect*, but not justly *conclude*, that more cases of the same variety might be found. If we found 18 *consecutive* analogous cases, although no *relevant* selective factor seemed to have been present, we might perhaps conclude that such cases cannot be altogether infrequent. *But* there would be no justification for the generalization that the same etiology is true of each and all cases of hysteria; it might be true of a specific subvariety of hysteria.

§385. Ninth, if such evidence as that pertaining to Victor had been obtained about even one patient, to say nothing of 18, *there could be no question of retracting the seduction theory with a stroke of a hand, and to substitute it with the theory that the patients had merely fabricated the events*. If the reality of the seduction events be doubted, the entire picture will fall to pieces, and one will have to start from scratch. Not the slightest amount of the evidence can in any apparent way be used as support of the fabrication theory.

Tenth, I venture the postulation that none of my readers is capable of imagining that Freud *could at all* have retracted the seduction theory, if he had obtained evidence of the power illustrated by the biography of Victor—as he ardently assures that he had.

§386. Eleventh, the application of a single-case method does *not* imply that only observations emerging within the consultation room may be used. Obviously, if the psychotherapist had obtained information about the content of the diary and the order and time series of the first appearance of the symptoms, from Victor's own verbal reports in the consultation room, the evidential power would be



null and void. The therapist would not even know whether the diary existed at all. A fundamental feature of the proof is that Victor's account may be checked against external sources which he could not have known about.

§387. Twelfth, one feature is of utmost significance for general methodology. *Two "facts", each of which is of doubtful or even extremely doubtful validity, may reciprocally confer complete validity upon each other.*

This feature is not restricted to the method of the jigsaw puzzle. But it may frequently be encountered when the latter is applied.

§388. We may ask whence the evidential power of the data of the biography derives. Evidently, it derives from the fact that the biography contains *parallel order relations*. It contains no less than three parallel series.

If I toss a coin 20 times and obtain 10 heads and 10 tails, this outcome is probably a chance phenomenon. However, if I find that the same (or the opposite) outcome is invariably observed for the 1st and 11th throw, the 2nd and 12th, the 3rd and 13th etc., then we can be sure that some causal factor is responsible for this pattern: either the temporally first series (the first 10 throws) is the cause of the subsequent series, or else some still earlier unknown series is responsible for both the observed series.

Readers with a liking for philosophical logic may with much profit compare this brief analysis with Reichenbach's (1970) proof of the existence of an external world in §14 in *Experience and Prediction*.

§389. Parallel series are easily seen to provide conclusive proof. But some reflection will certify that the opposite statement is likewise true. *No single-case method that does not involve parallel order relations, would have any chance of proving the seduction theory.*

I shall not present a formal proof of this statement.

§390. It is not a daring suggestion that no psychoanalyst could be found, who would be prepared to admit even *the possibility* that a pattern of data such as that of the biography of Victor, could ever be encountered in the empirical world.

This is an important fact. If no different kind of pattern could prove the seduction theory, and no psychoanalyst has observed any pattern that is even remotely similar, then no psychoanalyst has ever observed any amount of support of this theory. In particular, neither Freud and Ferenczi, nor Alice Miller and Jeffrey Masson, have done so, however much they may assert the opposite.

My result may be generalized: *no psychoanalytic interpretation at all could be verified by any single-case method which does not involve parallel order relations.*

In the present context I need not take a stand as to whether there is any interpretation that could be proved by any non-single-case method. For generations, psychoanalysts have unanimously agreed that they justify their interpretations by means of single-case methods. A few references selected at random may be mentioned: Freud (GW-I:441f./SE-III:205) (GW-XVI:43ff./SE-XXIII:255ff.), Fenichel (1945:31f.), Rapaport (1965:118), Kubie (1952:65ff.) (1960:502ff.), Sherwood (1969).

The consequence is inescapable. When psychoanalysts claim to be in the possession of genuine evidence of their interpretations, they are not telling the truth.

§391. Freud himself repeatedly tries to prove his interpretations by means of parallel order relations. In Q-16:1 we encountered the faked claim that Dora's attacks of loss of voice coincided with Mr. K.'s absences. In Q-14:1 as well as in numerous other places throughout *Gesammelte Werke*, there is either an explicit assertion or an unambiguous implication to the effect that hysterical symptoms will disappear over night, as soon as the original causal event has become de-repressed. And Freud states that, most patients will have more than one symptom deriving from more than one causal event. For such patients, parallel order relations could hardly fail to be observed, if the treatment is successful.

In Q-15:1 a weak parallel order relation is implied. Any individual suffering from an anxiety neurosis will recover over night, if he or she ceases to practice sexual abstinence, coitus interruptus, or coitus with the kind of condoms in existence at the turn of the century.

Freud does not claim that the individuals will necessarily or immediately relapse, if such orgasm-reducing practices are resumed. But the probability of relapse is evidently increased. At the same time: however many relapses such a patient may suffer, each of them will be associated with the resumption of such practices, and each of them will immediately disappear when sexual activity producing a full orgasm is resumed.

All these postulated parallel order relations are deliberately faked, as we have already seen. A few comments to the last one may be appropriate. It has for centuries been observed that sexual abstinence may sometimes cause neurotic disorders (while it may sometimes produce mere discomfort). On the other hand, I doubt very much that any psychoanalyst would today dare to suggest that Freud's hyper-simplified claims are true.

§392. Throughout most of the present chapter I have given a most embryonic account of the method of the jigsaw puzzle and the context of the latter. Nonetheless, this minimal analysis is enormously much more penetrating and informative, than any methodological section found anywhere in psychoanalytic literature.

During nearly a century, no psychoanalyst would have been able to tell the truth about what method he actually applies in his consultation room. But psychoanalysts have no choice except to claim that they apply *some* respectable method. An impressive number of procedures have been claimed to be *the unique* psychoanalytic method.

The claim that psychoanalysts apply *the experimental method* has been made by Fromm (1968:175) and Deutsch (1945:II:9). Heinz Hartmann (1959:21) partially agrees. Kubie (1960:502ff.), who does not use *the term*, actually goes further than any of the others. Numerous academic psychologists have accepted this claim. But in the long run the latter is ineffective: psychologists are too well aware of the specific features of a genuine experiment.

§393. By contrast, no standard text on the method of the jigsaw puzzle exists

and, as far as I have been able to find out, there is neither any non-standard text. Extremely few behavioural scientists have even heard of this approach, except as a mere name. Numerous linguists and historians are proficient in the procedure, when it is applied within their own domain. But they are not capable of seeing how it could or should be applied within psychology. (And they will not dare to use another rule which would, more often than not, lead them to the correct evaluation of psychological arguments, viz. that a derivation that is invalid within linguistics or history, is also invalid within psychology.) Consequently, extremely few behavioural as well as non-behavioural scientists are capable of pointing out that no trace of the method of the jigsaw puzzle can be found anywhere in psychoanalytic literature. What *is* found in this literature is the exact equivalent of the linguistic speculations of Romanticism. (An example. The Queen of Saba who visited King Solomon, was the queen of Hawai. The proof is based on the numerous instances in which "h" is found in classic Greek to correspond to "s" in Latin, e.g. hemi-/semi-.)

Psychoanalysts may say to skeptics and others: we psychoanalysts have arrived at our results by applying a specific method, which few non-psychoanalysts would call pseudo-scientific or non-scientific, and of which nearly everyone is ignorant.

## Freud's Specific Proofs of the Seduction Theory

*Solche Kinderverhältnisse sind, wie Sie gleich hören werden, gar nicht selten. [Hören Sie jetzt einen Beweis:] Ich bitte, verweilen Sie noch einen Moment bei der besonderen Häufigkeit sexueller Beziehungen im Kindesalter gerade zwischen Geschwistern und Vettern.*

Sigmund Freud

*Oft beweisen diese Herren durch ihre Beweise nichts, als dass sie das Beweisen hätten sollen bleiben lassen.*

Gotthold Ephraim Lessing

§394. Freud sets out to prove two claims. On the one hand, that the 18 patients did not fabricate, but recounted true descriptions of authentic seduction events. On the other hand, that there is nothing inherently implausible about the suggestion that sexual seduction of young children is not an infrequent occurrence.

One might think that: if Freud had really proved the former claim, he would not need prove the latter. By contrast, an unbiased reader might think that a third claim is much more important than either of these, viz. that the seduction events were causally responsible for the emergence of the symptoms. But Freud makes no attempt at proving this claim. He merely presents dogmatic assertions about the enormous power of secret evidence in his possession:

"[...] evidence which, if you were given the complete history of a case, would be as clear as daylight to you." (GW-I:446/SE-III:210) [Q-394:1]

§395. I would ask the reader to reflect for a moment upon what options would have been available to Freud. Since he had no evidence at all, he would not be able to present any authentic evidence related to any of the three claims. If he should choose to present anything, it would have to be *faked* observations.

Now, it is perhaps no unsurmountable task to suggest a set of circumstances which would, at least at the first glance, seem to prove the reality of the seduction events.

For instance, confirmation by external observers. But as regards the causal responsibility of the seduction event, neither Freud nor any previous commentator have had even an embryonic idea as to what kinds of facts could prove such a relation.

§396. I shall extract and discuss each proof advanced in the third seduction

paper, however trivial it may be. I shall start by juxtaposing two arguments: the most satisfactory and the least satisfactory ones.

The first of these has been sufficiently discussed in ch.15, viz. that the historical authenticity of the de-repressed infantile events has been confirmed by outsiders.

The second proof was used as an ingress to the present chapter. Freud promises to prove a certain circumstances, to skeptics who may doubt it. And then he invokes the authority of these skeptics: they need just recall that they know in advance that Freud is correct.

"Such relations between children are, AS YOU WILL HEAR IN A MOMENT, by no means rare." (GW-I:442/SE-III:206, capitals added) [Q-396:1]

"I will ask you to consider a moment longer the special frequency with which sexual relations in childhood occur precisely between brothers and sisters and cousins, as a result of their opportunities for being together so often." (GW-I:445/SE-III:209) [Q-396:2]

The considerable efficacy of this persuasive device should not be overlooked.

§397. According to his own testimony, Freud was not just gullible and believed what his patients told him. He did not in the least overlook the possibility that the patients might have fabricated, perhaps because of suggestive influence. Consequently, Freud performed a meticulous investigation of this point.

Thereby he gathered certain facts which "provide conclusive proof" and are "in every respect incompatible with the assumption that the scenes are anything else than a reality" (GW-I:440, 441/SE-III:204). The reader will recognize these formulations from Table 375:1.

The third proof may be presented in Freud's own words:

"Even after they [= the patients] have gone through them [= the infantile events] once more in such a convincing manner, they STILL ATTEMPT TO WITHHOLD BELIEF FROM THEM, by emphasizing the fact that, unlike what happens in the case of other forgotten material, THEY HAVE NO FEELING OF REMEMBERING THE SCENES.

This latter piece of behaviour seems to provide CONCLUSIVE PROOF. WHY SHOULD PATIENTS ASSURE ME SO EMPHATICALLY OF THEIR UNBELIEF, IF WHAT THEY WANT TO DISCREDIT IS SOMETHING WHICH—from whatever motive—THEY THEMSELVES HAVE INVENTED?" (GW-I:440f./SE-III:204, capitals added) [Q-397:1]

§398. This proof merits many comments. First, although there is nothing wrong about the English translation, some readers might miss the crucial point that is transparent in the German text ("nicht [...] ein ERINNERUNGSGEFÜHL eingestellt hat"). The meaning is that the patients have no *feeling* of their accounts being *genuine recollections* of events which *really happened*.

Second, by advancing *this* argument, Freud has irrevocably closed the possibility of later escaping by means of the substitute theory that *the patients* had merely fabricated, while Freud's mistake consisted in *believing that the patients told the truth*.

Here, Freud explicitly admits that the patients claimed *not* to have been sexually seduced.

Third, the most comprehensive description known to us about infantile seduction, is that of G.de B. (cf. §6). Let us compare Q-397:1 with this case.

a. G.de B. did certainly not go through the infantile event "once more", nor "in such a convincing manner". But it could hardly be more true that b. she "*still attempt[ed] to withhold belief from*" the event, and c. claimed to *have no feeling of the scene being a recollection at all*.

I shall not invariably repeat that this or that patient reaction postulated by Freud simply *could not* be authentic, since Freud himself had fabricated the seduction events. But *when the postulations of Q-397:1 are compared with the historical facts, we are provided with a most illuminative illustration of Freud's technique of fabrication. Various pieces of the authentic occurrences are included into the fabricated version. But they are deprived of their authentic context, and supplied with a new and completely fictive context of the opposite nature, so that the authentic pieces will appear to have the reverse character.*

Exactly this technique may be repeatedly found throughout Freud's writings from every period.

§399. Fourth, it is a well-known fact, and freely admitted by psychoanalysts, that many of their patients will call them "tricksters" and accuse them of using "confidence tricks". Numerous patients have testified that they have been the object of the second-order interpretation: "The very fact that you reject my interpretation, constitutes an unassailable proof that the interpretation is true." This variety of testimony is much too frequent to be disbelieved. And occasionally, the psychoanalysts themselves have, as it were by a slip of the tongue, admitted in print that they do apply this stratagem.

I think Q-397:1 is the very first instance in print of such an admission. Moreover, I wish to strongly emphasize, on the one hand, that the patients' accusation is based on a veracious perception of the authentic reality; and, on the other hand, that the psychoanalysts are aware of what they are doing, when they apply the third-order technique of interpreting the accusations as the result of "negative transference" with the concomittant distorted perception of reality.

§400. To sum up,

*first-order interpretation:* the fact that you suffer from oral eczema, proves that your father practiced fellatio upon you when you were in the cradle.

*second-order interpretation:* the very fact that you reject the first-order interpretation, proves that the latter is true. (Hence, there are now two independent and conclusive proofs of this interpretation.)

*third-order interpretation:* the very fact that you call the second-order interpretation a confidence trick, proves that you are a victim of negative transference and, consequently, distort reality to satisfy your pathological unconscious needs.

[Q-400:1]



Numerous patients who have undergone psychoanalytic treatment, will recognize this scheme. Evidently, it is immaterial what particular interpretation be inserted into the latter.

Likewise, only a grumbler would object that the case of G.de B. is taken from a private letter, and not from any of Freud's published writings.

§401. There are strong reasons to remain with the present problems. Note a specific persuasive technique—which in fact belongs to Freud's standard arsenal.

*Many layers of untruths of different kinds are involved in Q-397:1.* First, the patients never claimed to have been seduced. Second, it was Freud himself who fabricated the seduction events. Third, Freud ardently tried to induce the patients to accept these fictions. Fourth, the patients cannot have introduced the events, and cannot have gone through them, whether in a convincing or an unconvincing manner. Fifth, the patients did not say that such and such events occurred to them, while they merely doubted that it was a matter of genuine recollections. They said that they did not in the least recall anything even remotely similar to the events postulated by Freud. Sixth, at least some patients said, at least in the beginning, that they did not believe in Freud's interpretations.

This list might be continued. Many patients and researchers have, in various contexts, doubted *some* of the fabrications, but accepted others. This is a natural human reaction to labyrinthic texts. Since they may have doubted and accepted different patterns of fabrications, they have naturally arrived at discrepant conceptions as to what psychoanalysis really is. And psychoanalysts have ridiculed the discrepancy of these views.

It seems however that the overwhelming majority of the views are in better agreement with reality, than the psychoanalysts' own position.

§402. Now to the fourth proof. The "fact" that the patients *feel ashamed* of the events, provides per se unassailable evidence of the historical authenticity of these events (GW-I:440/SE-III:204). Here, a further aprioristic axiom is unambiguously implied: no human being could possibly feel ashamed of something he *believed* he has done, unless he had actually done it.

It is a matter of routine to invent counter-instances of this axiom, and I have presented one in Q-119:1, ex. 5.

The principle that individuals feel ashamed of what they *believe* they are or have done, even if their belief is altogether erroneous, is frequently applied within experimental psychology. For instance, subjects may be given fake GSR readings when exposed to pictures of nudes of their own sex.

On the other hand, I do not quite grasp *why* Freud's patients felt ashamed, since they did not have any feeling of recalling authentic events. Freud's text can hardly be construed as meaning that they felt ashamed of having certain kinds of *fantasies*.

We may tentatively infer that there is a certain degree of *isomorphy* between the distortions upon which the third and fourth proofs are based. Michael's and Florence's reactions may not have been basically different from that of G.de B. And Freud's description gives no indication of her having felt ashamed.

§403. Freud's *persuasive techniques* belong in a different volume. But many of the peculiarities of the proofs will be more transparent, if some of these techniques are described here.

*How did Freud manage to conceal the total absence of clinical evidence in his paper?* He realized that he must at the same time talk about something else, and feign that he is still talking about the same thing.

He feigns that he is wavering, as if it did not matter very much which alternative he would eventually choose. Should he present his evidence *first*, and discuss various expected objections *later*? or *vice versa*? He decides to discuss the objections first, because, as he says, when the objections have been answered (note the word "answered"), it may be possible to look at the facts in a calmer mood. (GW-I:439/SE-III:203)

AND THEN FREUD PROCEEDS TO DISCUSS A NUMBER OF OBJECTIONS. AND THE WHOLE PAPER COMES TO ITS CLOSE BEFORE HE ARRIVES AT THE PRESENTATION OF ANY KIND OF EVIDENCE.

§404. As if this were not enough, many of the objections are not answered at all. And not a single one among the *important* objections is answered.

For instance, the objection that the infantile events probably did not actually take place is, *inter alia*, answered by the dogmatic assertion that it has simply been proved by Freud's psychoanalytic treatment that they really took place.

Any genuine researcher who chose to give such an answer, would definitely have presented his evidence *before* he discussed the objections.

When the reader expects Freud to present evidence, Freud evades the task. He promises to do so later on, but just now he must answer objections.

And when the reader then expects Freud to answer the objections Freud himself has advanced, Freud evades this task by presenting evidence.

Or rather, he merely presents ghost evidence: he asserts that he is in the possession of secret evidence which proves that he is correct.

A deliberate strategy is evidently involved. *By feigning that he is not presenting evidence at all, Freud succeeds in presenting ghost evidence instead of real evidence.*

§405. The reader should note *the isomorphy* with other features of *Gesammelte Werke*. Allegedly, *it is always the most easy thing for Freud to present unassailable evidence. Only, because of a never ending flood of excuses, he can never do this in the paper at hand.*

In the second and third seduction papers Freud was concerned with *many* cases. He presented *the results* he had obtained, but he did *not* describe *the method* by means of which they had been obtained. Consequently, the reader was deprived of any opportunity of making a rational decision as to whether he had been presented with legitimately established facts or with idle speculation. Freud explicitly explained that the method must be justified by the close scrutiny of the observations of *a single case* (GW-I:418, 441f./SE-III:153, 205).

When Freud later published a single case-study, viz. that of Dora, he explicitly claimed that he would solely present *the results* he had arrived at, but would not

explain *the method*. And now he gave an explicit vindication of this decision: it is a feature of the psychoanalytic method, that it can *only* be justified by means of *many* cases. (GW-V:170/SE-VII:13).

§406. Another persuasive strategy, recurrently applied by both Freud and his followers, is to point out—correctly *or incorrectly!*—that psychoanalysis has been the object of two opposite varieties of criticisms. It is implied that these kinds of criticisms reciprocally disprove each other. Hence, both of them are invalid.

Some people may accuse astrology of neglecting the social determinants of human behaviour, while others may claim that astrology neglects the biological determinants. The social critics disprove the biological critics, and the biological critics disprove the social critics. Hence, astrology has not neglected any genuine determinants.

This stratagem is present already in the third seduction paper. Freud expects two opposite objections to his theory. On the one hand, infantile sexual events are much too infrequent. Hence, they cannot explain the phenomenon of hysteria, which is not altogether infrequent. On the other hand, infantile sexual events are much too frequent, and many individuals have engaged in such events without having suffered any negative consequences. Hence, they cannot explain the phenomenon of hysteria.

On the second objection Freud gives the satisfactory answer that sexual seduction is a *necessary*, not a *sufficient* cause. But the first objection is rebuked, *inter alia*, by the fifth proof, which has already been discussed but not yet given a number: It has simply been proved by psychoanalytic investigation that the sexual events really took place.

§407. Sixth proof:

“Patients sometimes *describe as harmless* events whose significance they obviously do not understand, since they would be bound otherwise to be *horrified* by them. Or again, they mention details, without laying any stress on them, which *only SOMEONE OF EXPERIENCE IN LIFE can understand* and appreciate as *subtle traits of reality*” (GW-I:441/SE-III:205, italics capitals added) [Q-407:1]

Read this quotation carefully. I shall try to leave no ambiguity as to the message Freud intends to communicate.

Suppose I utter the following sequence of sounds: “Waga yado ya nezumi to naka no yoi hotara”. I myself may fancy that I have just emitted a series of non-sense syllables. But an expert of oriental languages might realize that I had recited a Japanese poem by Issa.

In the same way a patient may emit a series of statements which, according to his own view, have no “hidden” meaning. A psychoanalyst may however realize that this series signifies a far from harmless event, viz. sexual seduction. But since the patient did not understand what he “really” said, he could not possibly have fabricated or faked his testimony of the seduction events. Consequently, there is no escape from the conclusion that these events actually took place.

§408. This is a non sequitur. Even if the premises were true, the invoked evi-

dence would not provide the least evidence as to whether the unconscious mind was concerned with historical events or fantasies; or, in the former case, whether these events took place when the patient was 2–4 or 11 years old.

Note also that the entire argument is vacuous. Freud pretends that he has asserted some definite claim. But actually he has asserted nothing. This technique will make the “claim” very hard to refute.

Evidently, no one but a psychoanalyst would “understand” that the presence of oral eczema proves the authenticity of the event of fellatio.

But it is strange to see the word “a psychoanalyst” substituted with “someone of experience in life”.

§409. Scrutinizing the patient biographies in §243, none of them contain any reference to a phenomenon which a patient might describe as harmless, while he or she would feel horrified if he understood what he said. Alice, Beatrice, Florence and Michael could not easily avoid understanding the meaning of what they (allegedly) said. What Desirée and Elsa (allegedly) performed, may well have been altogether harmless. Whether Michael and Florence had reason to be horrified, would depend upon details about which we are told nothing.

§410. However, it is far from impossible that we are confronted with one more instance of the specific technique described in §398. I surmise that the accounts really given by the patients, were harmless. But Freud reacted to these harmless descriptions as if they were Rorschach inkplot cards: he saw all sorts of sexual perversions in them.

Can any support of this hypothesis be found? Since the seduction papers are almost totally devoid of clinical observations, we shall not find any support there. But in the case-study of Dora there is an impressive number of isomorphic instances.

A female patient said about a small ivory box filled with sweets: “I always have this box about me; I take it with me wherever I go.” Freud says that he even *laughed* when he told the patient that her statement is indeed true, since her box means her vagina (GW–V:240/SE–VII:77). The reader may insert the patient’s statement into Q-407:1. The only non-trivial difference is that the patient might not have been horrified by understanding that she was talking of her own sex organ.

A second example. During a consultation Dora was playing with her hand bag; she repeatedly put her finger in it. This circumstance, and this circumstance alone, proves that she masturbated as a child (GW–V:238ff./SE–VII:76f.)

The reader may also recall from Q-17:1 and §766 how quickly Freud arrived at his interpretations of Dora’s cough attacks and asthma.

§411. Furthermore, Freud wanted to prove that the embryo of Dora’s wish of practicing fellatio was present already during early childhood. And he presented a proof:

“Dora herself had a clear picture of a scene from her early childhood in which she was sitting on the floor in a corner sucking her left thumb and at the same time tugging with her right hand at the lobe of her brother’s ear as he sat quietly beside her. Here we have

an instance of the complete form of self-gratification by sucking" (GW-V:211f./SE-VII:51) [Q-411:1]

Whatever we may think of the English formulation, the German word "Selbstbefriedigung" (self-gratification) could hardly fail to evoke the idea of masturbation.

It should be noted that such events may safely be fabricated about any patient. Moreover, numerous patients will recount events which may easily be distorted into the occurrence of Q-411:1. On the other hand, the description might be true; and I shall take it at face value.

The crucial point is that Q-411:1 provides an excellent example of an event satisfying Q-407:1. Dora described the event as harmless. She did not understand its significance. Only a psychoanalyst could understand the meaning, and derive information on "subtle traits or reality" from Dora's account. And if Dora had understood the significance of her description, she would most probably have felt, if not horrified, at least highly embarrassed.

It is no risky guess that Q-407:1 is indeed intended to refer to such trivial events as the one described in Q-411:1.

§412. We may now return to the opposite objections of §406, that events of infantile seduction are at the same time too frequent and too infrequent to explain hysteria. It seems that both objections are true. Events like Dora's sucking must be extremely frequent. Events like Michael's foot masturbation must be extremely infrequent.

I venture a hypothesis which many people might overlook. Where the latter kind of events do lead to psychopathological reactions, there is every reason to believe that it will in most cases produce syndromes other than hysteria.

§413. Seventh proof. The infantile events recounted by the patients are uniform in certain details. This uniformity would be impossible, unless the events had really taken place, or all patients had met in secret and agreed to deceive Freud. (GW-I:441/SE-III:205)

The reader may apply for himself the analysis given in §401, on the many layers of untruths involved here. But let us first take the argument at face value. Evidently, such uniformity could easily have derived from the psychiatrist's suggestive influence. And that much was generally known in 1896.

Freud himself later invented a quite different explanation: the patients had masturbated during infancy, but had repressed the memory of the true occurrences and distorted the latter into acts of seduction. From a logical point of view, this explanation is even more far-fetched than the original one. But many academicians think a theory is less offensive, if it attributes negative behaviours only to individuals who occupy a low position in the social hierarchy.—One circumstance is however apparent: it is a matter of routine to construct alternative explanations.

§414. Note that we are not told *what* details are uniform. Allegedly, some but not all patients felt ashamed. And some but not all considered their description



harmless. Apart from the fact that some kind of sexual activity was involved, and that all patients were 2–4 years old, I am not sure that any uniformity in detail can be found in the third seduction paper.

*Proving an assertion by the invocation of secret observations, is one of Freud's utmost recurrent techniques. And it is a standard technique used by numerous fabulators.*

Freud explicitly states that the case-study of Dora was published for the purpose of informing other psychiatrists about the nature of hysteria. We are told that Dora would immediately recover if her father left his mistress. The proof consists of the following words: "I WILL PASS OVER THE DETAILS WHICH SHOWED HOW ENTIRELY CORRECT ALL THIS WAS" (GW-V:202/SE-VII:42, capitals added). A second example: Mr. K.'s genuine wish of marrying Dora (a wish no unbiased reader will be capable of believing that he ever had), is proved by those *non-cited* words he said at Christmas (GW-V:271/SE-VII:108). A third example: "Certain details of the way in which she expressed herself (WHICH I PASS OVER HERE," prove that Dora considered her father sexually impotent (GW-V:207/SE-VII:47). It would be a matter of routine to list many more examples of the same kind.

§415. The eighth proof is not intended to establish that the seduction events really took place, but only that the patients' recounts could not have derived from Freud's suggestive influence.

"I have never yet succeeded in forcing on a patient *a scene I was expecting to find*, IN SUCH A WAY that he seemed to be living through it with all the appropriate feelings." (GW-I:441/SE-III:205, italics and capitals added) [Q-415:1]

Unless one or more hidden reservations be involved, this is a deliberate untruth. Freud certainly succeeded in forcing a scene upon G.de B. (cf. Q-6:1). But the words given in capitals could be such a reservation: Freud does not deny having succeeded in forcing such scenes upon a patient *in other ways*.

Numerous inconsistencies are involved. As we shall see in another chapter, Freud *denies* having had any expectations as to what kinds of scenes he might find. Moreover, HOW COULD FREUD TESTIFY THAT HE HAD NEVER SUCCEEDED, UNLESS HE HAD ACTUALLY TRIED? In particular, the adverb "yet" together with the use of an emphatic word order in German, are revealing: "Mir ist es noch nie gelungen...": "*To me, there has never yet been any success in...*"). This kind of a "slip of tongue" constitutes an important clue, according to witness psychology. However much Freud is fabricating, a part of the truth will creep in here and there.

Moreover, it seems to me that Q-415:1 does not agree very well with Q-397:1. How could anyone seem to be living through a scene *with all the appropriate feelings*, and at the same time *have no feeling of the scene being a recollection of an event which really took place*?

§416. I discussed the proof based on external verification in a separate chapter, and I shall basically do the same thing about the ninth proof, apart from a few



comments. Freud claims that the chains of (free) associations produced by the patients may in the beginning move in all directions. But they will eventually converge in a single point (or a few single points?), viz. in the event(s) which is the true cause of the hysterical symptom(s) (GW-434/SE-III:199).

Note that Freud invokes a variety of *non-esoteric evidence*, which could most easily be rendered in print without any loss of relevant information. Moreover, Freud is unaware of the strange nature of his account. We may have a patient or an experimental subject start chains of associations, by taking 5 different stimuli (possibly selected at random) as his points of departure. And during a 15-minute session, all chains may converge in the same event. But it is a quite different thing to have a patient produce free associations during 100 hours or more, so that the chains will converge toward the end of this period. In 1896 patients underwent psychotherapy for the purpose of being cured of their symptoms. There could be no question of symptom removal occurring around, say, the 50th consultation, whereafter the patient would have 50 additional hours to obtain various surplus qualities. And according to Freud's explicit testimony, symptom removal would immediately follow, as soon as the original causal event had been dug out.

During 100 hours, chains of associations would probably converge and diverge many times. But there could be no question of any final convergence which was not immediately followed by subsequent divergence.

§417. Even if the patients had fabricated the seduction events, it would have been a genuine and important discovery that chains of associations invariably converge in such an event. However, when Freud borrowed the method of free associations, he did not *know what to do with it*. In the case-study of Dora, there is a host of *Freud's own* associations to Dora's dreams. But he hardly managed to have her produce any associations, and certainly no *chain* of associations. The case-study of the rat man is almost maximally ascetic in this respect. Hence, it is simply impossible that the claim about converging chains of associations has any foundation in the empirical reality.

§418. Note also the discrepancy between the convergence argument and §350, according to which "the memory must be extracted from them [= the patients] piece by piece", "under the most energetic pressure of the analytic procedure, and against an enormous resistance" (GW-I:418/SE-III:153). Here, Freud, by a slip of the tongue as it were, tells a part of the truth. By implication, he testifies that the observation of converging chains of associations was never observed, but merely fabricated.

In fact, Freud describes in *Studien über Hysterie* a specific technique:

"I inform the patient that, a moment later, I shall apply pressure to his forehead, and I assure him that, all the time the pressure lasts, he will see before him a recollection in the form of a picture or will have it in his thoughts in the form of an idea occurring to him; [...] This procedure has taught me much and has also invariably achieved its aim." (GW-I:270/SE-III:270) [Q-418:1]

The reader may try to imagine Freud pressing the foreheads of his patients conti-

nually during 100 hours. Flagrantly, the technique described in Q-418:1 will not produce any *chains* of associations, but merely isolated fragments.

It is one of Freud's habitual untruths that he succeeded completely with this or that: *all* the 18 patients were cured of *all* their symptoms, and *all* of them experienced de-repression. Psychoanalytic treatment will cure almost *all* patients, and *all* these will be provided with a guarantee for life against relapse. The chains of associations converged in *all* cases. Etc. And now we learn that the pressure technique was successful *each time*.

§419. But it cannot be true that this technique was applied as often as Freud pretends. For instance, the fellatio event about G.de B. was not obtained in this way. And when she refused to believe in this interpretation, Freud did not demonstrate his confidence in the pressure technique: it did not occur to him to apply this technique and then let the facts themselves convince the patient.

Schusdek (1966) thinks that there was a close connection between the pressure technique and the seduction theory: Freud inadvertently influenced the patients to fabricate about having been sexually seduced. This idea is incompatible with Freud's text.

§420. I proceed to the tenth proof. Freud does not assert that the empirical facts upon which the proof is based, have been established; he merely makes a tentative suggestion.

Hysterics may be cured both *with* and *without* de-repression of the causal event. (We are not informed as to whether Freud himself has ever effected a cure of the second variety.) However, Freud "*presumes*" that there will be a risk of relapse after treatment not involving de-repression, while he "*expects*" that de-repression therapy will provide guarantee against relapse.

Let us disregard the fact that this presumption and expectation have been abundantly falsified. But we are once more confronted with a claim about having achieved superior results. The problem of psychoanalysis as a therapeutic technique will in the main be postponed to a later volume. But certain inconsistencies should not go unnoticed.

§421. In 1895, in *Studien über Hysterie* (GW-I:260/SE-III:262) Freud explains that a *causal* therapy will *not* produce symptom removal. It is merely a *prophylactic* method: it will prevent further injury, but will not cure the injury that has already happened.

A part of this idea is repeated in the case-study of Dora, published in 1905. "It may be safely said that during psychoanalytic treatment the formation of new symptoms is invariably stopped" (GW-V:279/SE-VII:116).

In 1917, in *Vorlesungen*, the implication is unambiguous that psychoanalytic treatment is free from the defect that only some patients will recover. In addition, this variety of therapy will produce a guarantee for life against the emergence of any psychopathological symptom (GW-XI:467ff./SE-XVI:448ff.)

In 1932, in *Neue Vorlesungen*, the therapeutic effect of a psychoanalytic treatment is said to be inferior to that of a visit to Lourdes (GW-XV:164/ SE-XXII:152).

In 1932, in *Konstruktionen in der Psychoanalyse*, we are told that: if a delivered interpretation is true or approximately true, the patient will react with an unmistakable deterioration as to his symptoms and general condition (GW-XVI:52/SE-XXIII:265).

§422. The eleventh proof is primarily intended to establish the “fact” that sexual seduction of children is a far from infrequent phenomenon in the general population.

Freud starts out with the statement that he has been *told* that there exist a number of publications written by *child physicians*, in which wet nurses and child nurses are accused of having performed sexual acts on babies and infants. He proceeds to say that: *if he be mistaken it doesn't matter, because one must “expect” that the considerable prevalence of such acts will soon be confirmed.* (GW-I:443/SE-III:207).

This “proof” is prefixed with the following sentence: “Let us begin our defence with the easier part of the task.”

Freud supplies one single reference, *Wilhelm Stekel (1895): Koitus im Kindesalter*. Freud is perfectly aware of the fact that Stekel is no child physician at all. He is a psychiatrist who, exactly like Freud himself, infers infantile sexual acts from symptoms or accounts presented by adult patients. In fact, the interpretation about Michael's stiff leg is as much in Stekel's as in Freud's style.

§423. Freud states that it was *colleagues* (pluralis!) who told him that there exist a number of *publications* (pluralis!). Although Freud had time to write three papers on the seduction theory, “I have not had time to collect other published evidence” than Stekel (1895) (GW-I:444/SE-III:207). *Recall: it is one of Freud's recurrent persuasive techniques to put his fabrications into the mouths of other people.* Allegedly, it was *Dora, not Freud*, who thought that her father was sexually impotent. *Dora, not Freud*, interpreted the fact that her mother suffered from white vaginal discharge, as evidence that she had syphilis and had been contaminated by her husband. *Dora* took the stomach-gripes of her female cousin as a sign that the latter was a masturbator. Another *cousin* interpreted Dora's paleness at the sight of Mr. K. as evidence of violent love. (GW-V:219/SE-III:59)

In a later volume I shall say something about *Margaretha Kossak (1913): Sexuelle Verführung der Kinder durch Dienstboten*. It may certainly be instructive to take a close look at the *only* psychoanalytic paper which, as far as I have been able to find, has confirmed Freud's observations about infantile seduction.

In a footnote in SE-VII:207 James Strachey claims that Stekel did not hear of Freud until 1901. The psychoanalysts' own version of their history is replete with the most incredible myths. Stekel and Freud were psychiatrists. Both were active in Vienna (how many psychiatrists were there in Vienna in the 1890s?). Both were particularly concerned with the deduction of infantile sexual phenomena from adult symptoms and patient accounts. Freud published his paper in *Wiener Klinischen Rundschau*, and Stekel published his in another periodical, *Wiener medizinische Blätter*. These papers were published more or less at the same time, 18.4.1895 and 21.4.1896. And Freud explicitly referred to Stekel's paper in his own.

§424. Stekel (1895:247) makes it clear that he is *not* going to talk of sexual seduction of children by adults. He will be concerned with sexual play between children. Let us see what he writes:

“Childhood is the bridge that connects homo sapiens with the animal kingdom. So, for instance, may *Lombroso* perceive in every child certain signs of the criminal because, just like the embryo during the first months represents the lower animal species, the child will during the first years represent the most inferior human type.

Hence, most children will spontaneously, on account of the sexual instinct, discover the coitus of childhood. Cases in which children have been abused by older persons, are well-known and do not belong to the context of this presentation.

It is doubtful whether a complete intromission of penis really takes place at such attempted or repeatedly performed coitus. For the most part the sexual act takes place in the vulva. Some of my observations seem however to indicate a partial intromission of penis in vagina. The possibility should not be rejected on apriori grounds. The necessary solidity is by no means absent from the erect child penis.” (Stekel, 1895:247, transl.) [Q-424:1]

The reader may evaluate Stekel’s trustworthiness for himself. But the irrelevancy of his results to Freud’s problem is apparent:

“As regards the health of the children, coitus per se does not seem to produce any noticeable harm. A part of my observations in this respect pertains to healthy just as well as neuropathy males.” (Stekel, 1895:248, transl.) [Q-424:2]

A brief summary of certain other aspects was given in Q-115:1. A physician claims that he and his girl-friend had practiced coitus once a week during pre-school age. But then: “*One day my girl-friend announced that she had started school, and that such ‘foolish actions’ were not fitting for a student.*” (Stekel, 1895:248, transl.)

Even if we grant that the most unique occurrences may occasionally happen, unsurmountable problems remain. Stekel mentions only two patients who presented such accounts. But his paper ends up with a *general* prescription about *all* children:

- “1. Children of opposite sex should under no circumstances sleep in the same room.
2. Children of more than 4 years of age should not share the bedroom of their parents.
3. Children should under no circumstances be taken into the bed of a maid or a gouvernante.
4. Children of opposite sex should never be allowed to play in dark places and never without surveillance.
5. Boys should frequently be examined for erections at night.
6. A child should only go alone to the lavatory. It should be forbidden to stay there for a protracted time.
7. Children of opposite sex should never at the school be taught in the same class.” (Stekel, 1895:249, transl.) [Q-424:3]

§425. I do not think there is any ground for the view prevalent among Freudians, that Stekel’s interpretations are more “wild” than Freud’s.

If the physician just mentioned was really responsible for the account attributed to him, the simplest explanation is evidently that he mechanically repeated the

interpretations Stekel had induced him to accept. It is improbable in the extreme that two individuals would independently arrive at the very same far-fetched idea, and would then meet and collaborate around exactly this idea.

I shall not discuss Stekel's merits and short-comings. The crucial point is Freud's mis-citation of Stekel's paper.

The statements made by patients in the consulting room are of an ephemeral nature. A printed text is a hard fact, and may be repeatedly checked if understanding or recall are deficient. One may be severely compromised by distorting the content of a published paper. By contrast, distortion of statements by patients could be challenged by at most one witness—and even a witness whose testimony may with the stroke of a hand be rejected as deriving from “negative transference”.

§426. We have now worked through all 11 arguments advanced by Freud. Nine of these were said to establish the reality of the seduction events. In actual fact, three of the proofs (no. 2, 5 and 6) consist of pseudo-scientific deductions. As for all the remaining ones, the empirical observations upon which they are based are deliberately fabricated. It is impossible that Freud could have been in good faith, when he made postulations of external confirmation, convergence of chains of associations, and therapeutic effect.

If the invoked observations had been authentic, the first and tenth proofs, and only these, would have provided some support.

It is absolutely impossible that he would have disseminated such proofs as the above listed ones, if anything better had been available to him. Moreover, if Freud had been in good faith, it is a sheer impossibility that he could not have produced support which was at least less conspicuously invalid than the ones included in his paper.

I venture the following statement. Even if we had no other evidence than these 11 proofs, this evidence would suffice to establish the conclusion that Freud was perfectly aware that his seduction theory had no foundation in any clinical observations of his.

## Freud's Theory of Want of Sexual Orgasm as the Cause of Anxiety Neurosis

*Freud was skillful in analyzing what would and what would not constitute evidence for and against his hypothesis.*

Clark Glymour

*... eine Beweisführung, die etwa an die plattdeutschen Märchen denken lässt, in denen jemand vom Gnomen fürsten träumt, der da auf dem morschen Ast ritt und dann herunterpurzelte. "Und zum Beweiss dessen, dass die Geschichte wahr ist — hier ist der Ast."*

Kurt Tucholsky

§427. So far, we have been concerned with Freud's fabrication and distortion of observations about the patients: their symptoms, their accounts, their infantile recollections, their recovery.

We shall now take one further step and ask whether the patients themselves may sometimes have been fabricated out of thin air.

In the following chapter we shall discuss whether the number of patients in the April sample really were 18. In the present chapter we shall see Freud fabricating empirical generalizations ad hoc, according to the momentous strategic advantage, without caring the least whether these generalizations may contradict each other. When Freud made the most firm postulations about the nature of the observations he had made upon *his own* patients, it could hardly be more flagrant that he had no specific patients in his mind. Nor did it occur to Freud how sizable the total number of patients must have been, if he had treated plenty of patients belonging to each one out of a whole series of classes.

The fabrication of patients in Freud's Löwenfeld paper of 1895, is the most central subject of the present chapter. But I shall also scrutinize other aspects of the Löwenfeld paper.

§428. In the beginning of 1895 Freud published a paper on the etiology of anxiety neurosis (GW-I:315ff./SE-III:85ff.). His assertions were immediately challenged by Löwenfeld (1895), who presented a series of cases from his own practice. About half of them did not agree with Freud's theory. Still in 1895 Freud (GW-I:357ff./SE-III:119ff.) published a reply, in which he also elaborated his theory. I will discuss primarily the reply. Freud's first paper will be analyzed in the fourth book.



The elaborated theory is nothing but a mechanical transposition of the usual theory of tuberculosis. In Freud's own text:

"I will try to give an example of this complete aetiological schematic picture:

*Effect:* Phthisis pulmonum.

*Precondition:* Disposition, for the most part laid down through heredity, by the organic constitution.

*Specific Cause:* Bacillus Kochii.

*Auxiliary Causes:* Anything that diminishes the powers—emotions as well as suppurations or colds.

The schematic picture for the aetiology of anxiety neurosis seems to me to be on the same lines:

*Precondition:* Heredity.

*Specific Cause:* A sexual factor, in the sense of a deflection of sexual tension away from the psychical field.

*Auxiliary causes:* Any stock noxae—emotion, fright, and also physical exhaustion through illness or over-exertion." (GW-I:373/SE-III:136f.) [Q-428:1]

In a second model (GW-I:372f./SE-III:135f.), Freud explicitly states that there could never be an anxiety neurosis without the presence of the hereditary precondition. Likewise, the specific cause, i.e. want of orgasm, is never absent. *The precipitating or releasing cause* is the only one showing temporal proximity to the emergence of the neurosis. It may consist of emotional affects. However, there need not be any particular precipitating event: the sexual tension itself may reach such a strength that it releases the symptom. *Concurrent causes* are neither necessary nor sufficient; they are merely facilitating.

§429. First a digression. Recall the wealth of pseudo-arguments disseminated during the last 25 years, about the unbridgeable gap between natural and behavioural science: experimental psychology is claimed to be perverse and to yield nothing but misleading results *because* it is said to imitate natural science, and psychoanalysis is said to constitute a satisfactory alternative because, it is said not to imitate natural science.

§430. A second digression. Although Glymour (1981:264) confuses Freud's papers on the seduction theory and on the want-of-orgasm theory, there is little doubt that the excerpt I have used as an ingress to the present chapter, is intended as a comment to Q-428:1 and the immediately following second model. However, Freud's paper is not in the least concerned with what would constitute support of anything. Freud merely presents a list of 4 or 5 abstract categories of evidence, together with the claim that he is in the possession of conclusive evidence related to all categories. He is silent about what evidence he has. In fact, he shows no awareness of the enormous problems which must be solved in order to obtain any evidence at all within his categories. And he is unaware of the improbable (impossible?) nature of his account.

§431. One of the patients mentioned by Freud was a man who developed an anxiety neurosis when his father died, and who had also practiced coitus interruptus for 11 years. In order to arrive at a conclusive and veracious estimation of the relative contribution of either factor, either extensive statistical investigation or

derivation from a theory previously confirmed by experiments, is needed. But on the basis of the temporal relations as well as lay knowledge of human nature, the better guess is that the anxiety neurosis would have emerged after the death alone rather than after the sexual practice alone. However, in Freud's view the father's death was a mere trivial cause, while want of orgasm was the genuine cause.

It is important that this example was presented *in 1895*, while the theory associated with it was presented *twice* during this year. Freud's own father died in October 1896. Later, Freud described this *type* of event as "the most important event, the most poignant loss, of a man's life" (GW-II:x/SE-IV:xxvi). Freud was incapable of perceiving what a situation would feel like, as long as he himself had not experienced it. He had little empathy, and also little self-knowledge, since he was convinced that the reaction he actually felt one year later *cannot* occur in any man. However, he immediately projected his new reaction on *all* males, overlooking the fact that there is considerable individual difference in reactions upon the death of fathers.

§432. Freud has allegedly discovered that hereditary factors are never missing in cases of anxiety neurosis, while such factors alone are never sufficient to produce this syndrome. Now, to disclose hereditary factors is by no means as easy a task as Freud believes. First, the data must be collected. One of Freud's patients was 54 years old. I am slightly older. When trying to tabulate my grandparents together with all their offsprings for two generations, I must confess my utter ignorance as to whether any of these individuals ever suffered from an anxiety neurosis. The reader may try to list his own relatives, *inter alia* estimating the incidence of frigidity. But allegedly each and all Freud's patients were capable of supplying veracious information about so many relatives, that a genuine zero-incidence or a biologically significant incidence could be established for each and all of them.

Freud (GW-I:364, 374/SE-III:129, 137) postulates a *hereditary frigidity*, and for some reason or other he does not mention the possibility of any other etiological type of frigidity.

§433. Freud's basic methodological principle is simple (GW-I:371/SE-III:134). A high incidence of a trait among relatives proves, and a low incidence disproves a hereditary etiology. This principle could hardly be more erroneous. It is well-known that beautiful family pedigrees may be collected of Germans who never use the grammatical imperfect of verbs. And I am the only colour-blind individual among all my relatives. Moreover, if 90% of a patient's relatives felt strong anxiety about sex, environmental causes might certainly be responsible if he himself felt the same thing.

Considering the many and grave contradictions throughout the paper, we cannot be sure whether the following statement is intended to bear on the present problem: heredity will not lead to any *specific* mental syndrome or symptom, but merely to one or another such entity. This position will facilitate data gathering, although by no means to a sufficient degree. And it may impede data interpretation. If a frigid female has a schizophrenic, a compulsive, and a homosexual rela-

tive, it is not obvious how these data might reveal whether her frigidity is hereditary or not.

In a later volume we shall encounter Roudinesco's (1982) strange fabrication that Freud allegedly was met with strong opposition in France, because hereditary views allegedly were prevalent in France, while Freud allegedly entertained a *non-hereditary* view.

§434. The Heredity Juxtaposition (Q-15:1) consists of statements from the second paper on the anxiety neurosis. Please re-read it! Note, Freud does *not* use the term "hereditary" about genetic properties which are common to all human beings.

There are no less than 11 contradictions between these 11 statements. Obviously, Freud does not describe authentic observations made on existing patients. He invents empirical generalizations *ad hoc*. And he does not care whether they are compatible.

He claims that he did not supply figures in his first papers, but "examples" instead. And the reason was that it was *not* his aim to convince his readers. His aim was merely to prepare their expectations (GW-I:358). The implication is unambiguous that he *would* have presented figures if it had been his aim to convince.

Now, in the second paper Freud's aim is flagrantly to convince the readers of the truth of the content of the first paper. Nonetheless, there are still no figures.

Furthermore, the aim of the first paper *is* to convince. And Freud has in the latter supplied few "examples", each of which is singularly lacking as to details. The paper consists primarily of a wealth of empirical generalizations on such a considerable number of categories, that few doctors would have had the opportunity of observing such a variegated sample during a life-time. Freud was 38 years old when he wrote the paper.

Moreover, the postulated generalizations are enormously much more contradictory, than the content of the seduction papers. In addition, the observations are psychologically impossible.—But since a considerable part of the second volume will be devoted to the analysis of the two papers on the anxiety neurosis, I shall say no more about this subject now.

§435. What has impressed Glymour is probably that Freud has formulated his inventions in a *jargon* which bears a superficial resemblance to John Stuart Mill's methodological requirements.

Freud even postulates a hereditary mechanism. The hereditary factor will act as a *multiplicator*, increasing the amount of any tension there may be.—Now, even if we would accept most of Freud's claims, it would still be highly surprising that this is the only mechanism. In fact, Freud forgets that he himself has described a quite different way of influence: what distinguishes females who, because of hereditary factors cannot have a sexual orgasm, is rather the continual accumulation of sexual tension, because the latter cannot find an outlet.—If Freud had had real observations made with real patients in mind, he would hardly have forgotten them from one page to the next.

§436. Although Freud does not supply any formula, it is easily seen that he is

thinking of a formula like the following one. (Nothing depends upon the exact numbers; exponents or roots might just as well have been proposed. And perhaps it is not Freud's obligation to have very precise ideas about the relations between the quantities.)

$$\left[ h \left( s + \frac{r}{30} + \frac{f}{100} \right) \geq t \right] \rightarrow A$$

s is the increase of tension deriving from sub-optimal sexual behaviour; r is the increase deriving from trivial releasing events; and f is the increase deriving from mere facilitating factors. h, which is a constant for each individual, is the hereditary multiplicator, and t is the threshold. A is the anxiety neurosis. If the quantitative product is equal to or greater than t, an anxiety neurosis will result.

If h is sufficiently small, no empirically possible values of s, r or f can raise the total quantity above t. If h is either normal or great, and if s is sufficiently great, then it is of no significance how great or small r or f are.

I do not think there is much point of constructing such quantitative theories until there is some way of connecting them with empirical phenomena. It is however interesting that Freud and psychoanalysis are considered satisfactory by most people who want to abolish all quantification from the behavioural sciences.

§437. I shall add two further sets of comments. First, Felix Gattel (1898) tried to test Freud's theory upon 100 patients. And 59 of these had an anxiety neurosis, according to Gattel's diagnosis. As we have seen, Freud accused him of having plagiarized Freud's theories.

*What is the minimum number of relatives about whom one must have the relevant information, in order to safely conclude that a patient's ailment is hereditary?* I think I am very generous in accepting a mean value as low as 5. Five times 59 is equal to 295 relatives. If Gattel had plagiarized Freud, he had obtained a total of at least 295 relatives with relevant psychopathological syndromes.

I think I shall leave the reader to *guess* how many afflicted relatives Gattel dug out. The answer will be stated toward the end of the present chapter.

§438. The second set of comments will be concerned with the paper on the anxiety neurosis which Löwenfeld criticized.

Around the turn and beginning of the century a theory was widespread within many sciences and non-scientific fields, which may seem odd today: tension is unpleasant and relaxation is pleasant. People who stated that sexual tension is unpleasant, can hardly have compared the theory with their own experiences. It was also an axiom that living organisms aim at getting rid of any tension they may feel.

As usual, Freud joined a popular vogue. In GW-I:334f. he presents an unoriginal theory. The male organism will, probably continually, produce male semen. The accumulation of semen will increase pressure on the nerves. And it is *this* circumstance that produces libidinous tension. Such tension must be released by the *adequate* action, to wit, by sexual intercourse. Masturbation will not do,

because it will not release all psychic tension into which the somatic tension has been transformed.

§439. Thousands of males must have had experiences that are difficult to reconcile with this theory. Sometimes they have had intercourse two hours ago, see a wonderful girl, and feel like exploding. Sometimes they have not had intercourse in 10 days, and nonetheless feel no more than a moderately strong urge. The strength of our drive at any moment is as much a function of external stimuli, as of continual physiological processes.

Freud adds that his model is likewise applicable to females, although he admits that he does not know *how* it should be applied.—But if some as yet unknown substance is continually produced in females, which continually increases pressure on the nerves; how do we know that some as yet unknown substance other than semen is not the physiological factor responsible for male sexual drive?

Tension released by the incomplete way of masturbation, is thought to cause neurasthenia. Anxiety neurosis is instead the result of three kinds of sexual behaviour: abstinence, practicing coitus interruptus, or practicing intercourse with condoms (of the kind available in the 1890s).

§440. Let us now see how many relatives with psychopathology Gattel (1898) found among his 59 anxiety neurotics.

One patient had a brother with paralysis.

One patient had a father with paralysis.

One patient had a brother who had died in a mental asylum; we are told nothing about the nature of his illness.

One patient had a mother who had migraine.

One patient had two parents who were “nervous” (but it possible that this term had a more concrete meaning in 1897).

Surprisingly, Gattel overlooked the fact that one patient had an aunt who likewise had neurasthenia; both had at least one common symptom, viz. headache.

Summing up, *1 patient listed a total of 2 relatives, 5 patients listed 1 relative, and the remaining 53 patients listed none at all.*

Obviously, the syndromes of 5 out of these 7 relatives are irrelevant. The syndromes of the remaining 2 relatives are of unknown relevancy.

The reader may judge for himself whether it is believable that Freud would in a comparable sample have dug out 4100% more relatives. Note that we are concerned with information, which is directly available without de-repression.

## How Many Patients Did Freud Have?

*Du musst verstehn!  
Aus Eins mach' Zehn.  
Das ist das Hexen-Einmaleins!*

Johann Wolfgang von Goethe

§441. The Löwenfeld paper is by no means the only writing in which Freud fabulates about “his numerous patients”, without having any real patients in mind. He has in many other papers produced empirical generalization, ad hoc and according to the need of the situation. We saw in Q-17:1 how Freud suddenly recalled that he had already verified upon “many” cases that patients had contracted asthma due to spying on their fathers with sick lungs having sexual intercourse.

On 21 April 1896 Freud claimed a. that he had cured all patients in the sample of all their symptoms; b. that all patients had experienced de-repression; c. that their recovery was closely associated with the occurrence of de-repression; d. that all of them claimed to have been sexually seduced during infancy; e. that no amount of suggestive influence had been present in the consultation room; f. and that the sample consisted of 18 patients.

We have seen waterproof evidence to the effect that each and all claims except the last one, were maximally discrepant from the authentic state of things. We must now ask: did Freud at least tell the truth about the number of patients?

§422. *There is considerable similarity between Sigmund Freud and Cyril Burt. Both of them were completely devoid of any empirical evidence. Both of them made extraordinary claims of being in the possession of empirical evidence of excessive power. Both excelled in invectives against researchers who held discrepant views.*

Moreover, the untruths of both are so clumsy that half an hour is sufficient to expose them. Nonetheless, neither Freud nor Burt were exposed during their lifetime. And both experienced an immense success.

The best summary about Burt's fraud may be found in ch. 11 in Broad & Wade (1983): *Betrayers of the Truth*. He claimed to have tested the IQ of twins adopted at birth. It turned out that there was no correlation between the IQ of the children and their adoptive parents, but a very high correlation between the IQ of the children and their biological parents.

After his death, it was disclosed that Burt's pairs of twins did not exist at all. But the really important thing is this. The fact that his data had no empirical root could be directly seen from his published writings.



*The number of twins reared apart were steadily growing.* Burt's first claim was that he had exactly 20 pairs. His second claim was that he had "*more than*" 30 pairs; that is, he did not even supply any determinate number. His third claim was that he had 53 pairs. Evidently, round numbers look suspicious.

§443. One might try to save Burt's honesty by means of the suggestion that further pairs were added to his sample. But such an indeterminate number as "more than" 30 pairs cannot be saved by this device, when Burt presents correlation coefficients with three decimals.

It is hardly possible that anyone could have found a zero correlation between adopted children and their adoptive parents, since adoption agencies are known to try to match the adoptive family to the biological parents.

But if Burt told the truth, he found a statistical miracle. In all his three samples of different size, the correlation between the IQ of the children and their biological parents was 0,771—a figure with three identical decimals. Such things never occur in the empirical world. And among 60 correlation coefficients asserted by Burt, there are no less than 20 such miracles.

§444. Turning now to Freud, we are confronted with an exact analogon of Burt. To restrict the scope of my report, I have tried as far as possible to analyze only the third seduction paper, and have until this point not even mentioned the first one. This strategy can no longer be maintained. Both the first and the second papers were posted for publication on 5 February 1896.

In the first paper, Freud claims to have successfully cured 13 hysterical patients, two of these being males (GW-I:380f./SE-III:163). The third paper is peculiar. The *first* time any number is mentioned, the indication is "*etwa achtzehn Fällen*" ("*some* eighteen cases") (GW-I:435/SE-III:199, italics added). Everywhere else in the same paper we are told that the number is exactly 18. And they consisted of exactly 12 females and 6 males.

A physician may legitimately be in doubt as to the exact number of patients he has treated or cured during a certain temporal interval. He may have forgotten some individuals he saw a long time ago, or saw recently for only a moment. He may wonder whether he can justly be said to have *treated* a patient he saw only for 10 minutes, or another patient whom he "treated" regularly for months, but who neglected all his prescriptions. For others, he may not know the exact outcome of the treatment, or he may not be sure whether a certain outcome should be termed "a cure".

*One-step arguments* may be constructed upon such irrelevant circumstances. According to Freud's own testimony (including his letters), a. he had not yet met 16 of the 18 patients 171 calendar days before the third seduction paper was read in public; b. the treatment of the remaining two patients was not so near completion that their recovery was in view; c. each and all 18 patients were treated for a long period, and most of them had at least 100 consultations; d. Freud was not in the least in doubt as to how many patients he had successfully cured on 5 February; hence the vagueness could only refer to the 5 newcomers of the last 10 weeks; e. Freud knew the exact outcome of the treatment for all patients; f. each

and all of the patients had experienced indisputable and complete recovery.

§445. Evidently, “around 18 cases”, “approximately 18 cases”) (more exact equivalents of the German expression), are not the kind of expressions that could easily derive from carelessness, in a researcher in the situation in which Freud claimed to be. We are concerned with a *deliberately* selected expression. And we need only ask: *What* deliberate motive could possibly have lead to this formulation. Not much reflexion is needed for realizing that only one explanation will hold water: *it is not true that Freud had exactly 18 cases.*

Nor is much reflexion needed for ascertaining that: if the real number of cases was *greater* than 18, Freud could have had no motive for using the vague formulation. Hence, the true number was less than 18.

§446. Strictly speaking, two caveats must be discussed. The first may easily be exposed of. Two fundamental types of hypotheses are possible.

Freud was perfectly aware of the fact that none of his patients had recovered; that none of them had experienced de-repression; that none of them had claimed to have been seduced that few if any had been treated for 100 hours. (*These are hard facts.*)

*The pure number hypothesis:* Freud reflected something like the following, without having any particular patient in mind. Should I fabricate that I have cured 5, or 10, or 15 patients? The greatest number will look most impressive, but the least figure is the least dangerous one. Where is the optimal cutting point between these two factors?

*The biography-distorting hypothesis.* Freud went over his patients, when he tried to figure out what number he should fabricate. He reflected something like this. Nathan saw me only three times. He did not recover. Should I include him in the list and pretend that he had had more than 100 hours, had recovered, and had de-repressed an event of infantile seduction?

[Q-446:1]

I have no hard evidence which may enable us to distinguish between these two hypotheses. My own choice is the former, but the reader should be free to prefer the latter. Various considerations may however throw some light upon the relative plausibility of the hypotheses.

First, the reader may try to reflect upon the question which approach he himself would choose, if he had to apply one of these, and had no additional option.

If there is a risk of being cross-examined, the second approach might be preferable (because of obvious reasons). But this risk was virtually non-existent. If someone got access to “my” files, I would be exposed in either case. In fact, I might be more easily exposed, the more features were known as to what particular patients there were to look for. It would therefore seem that the second approach merely differs from the first by containing a superfluous and redundant detour. That is, to a “normal” fabulator there would be no reason for preferring it.

As for Freud himself: whenever we can infer his actual reasoning from his

writings, it is apparent that he applied *highly inexpensive* techniques of fabrication. Hence, it is unlikely that he would make the detour.

§447. The second "caveat" is related to the content of ch. 3. On the one hand, *it is one of Freud's recurrent persuasive techniques to introduce his fabrications in gradual steps*. On the other hand, I strongly reject the prevalent idea that there is a distinct cutting-point between the complete awareness of lying and the total lack of awareness of not telling the truth. As I have already said: it is possible that Freud started out with the final version of the spying event (cf. Q-17:1), constructed two reduced versions, and presented them in the ascending order, for the purpose of increasing the likelihood that the reader would accept the final version. But it is more probable that Freud started out with the version involving the least empirical commitment (viz. that it is no more than a tentative hypothesis based upon indirect signs that the spying event took place at all). And that, as the result of having written down this version (with the decision of publishing it), his adaptation level was changed, so that he got more courage to proceed one further step; etc.

If we had no evidence except what is included in Q-17:1, we would have little reason to prefer one or the other explanation. However, the spying event is preceded by 8 proofs of Dora's childhood masturbation (e.g., Dora suffered from white vaginal discharge; and this symptom is usually the result of masturbation). I feel myself unable to imagine that Freud would have taken so much trouble of constructing so many arguments of this kind if he had known when he wrote them, that he would a few pages later fabricate that Dora herself had *recalled* that she masturbated when she was 8.

§448. Consequently, I think that Freud first fabricated that his sample consisted of 13 patients, because this was the maximal number he dared to assert. But having asserted this number, his adaptation level was changed, and he dared increase the number. He was however reluctant to assert as high a number as 18, and therefore used the formulation "*approximately 18*".

The exact degree and *quality* of Freud's awareness of not telling the truth, is a subject that cannot be solved at the present stage of our knowledge. But this problem may legitimately be postponed.

It may be more important to juxtapose a series of concrete instances of gradual introduction of fabrications:

1. The spying event was first a tentative hypothesis based on indirect signs, then a proven fact based on indirect signs, and finally an observed datum recounted by Dora herself. (Cf. Q-17:1)

2. The coincidence of the periods of Dora's mutism and Mr. K.'s absences was first a tentative hypothesis pertaining solely to a brief section at some unknown time long ago; and then it became a proven fact which was true of all periods. (Cf. Q-16:1)

3. In 1896 it was a tentative hypothesis that psychoanalytic treatment will produce a guarantee for life; in 1917 it was a firmly established fact. (Cf. §240)

4. In the second seduction paper, Freud placed a question mark after the indication that the infantile event had taken place as early as when the patient was two years old. No such question mark is found in the third seduction paper. (Cf. Q-285:1 and §345).

5. In *Charakter und Analerotik* from 1908, a paper which will be extensively analyzed in a later volume, Freud states that he is not going to prove the anal theory, but merely to make the latter somewhat more understandable. (GW-VII:206/SE-IX:172) This was perhaps an inescapable strategy, since the proofs are even more flagrantly invalid than the proofs of Dora's childhood masturbation or the proofs of the reality of the seduction events.—But in *Das Unbehagen in der Kultur* from 1930, Freud explicitly states that he has proved the anal theory in the paper of 1908. (GW-XIV:457/SE-XXI:97)

[Q-448:1]

It would be a matter of routine to extend this list to any desired length.

§449. We have already encountered *the simple criterion of isomorphy* (cf. §299). If two statements are 1. disseminated by the same person, and are 2. isomorphic; and one of them is 3. known to be false and, 4. known to be deliberately false; then we are entitled to conclude that the other is probably likewise deliberately false.

In the present case we have 6 statements, all of which are isomorphic, and 5 of which are known to be deliberately false. The probability of the last statement being likewise deliberately false, is therefore so high that we may skip any reference to probability.

§450. Further comments to the pure number hypothesis and the biography-distorting hypothesis are appropriate. If the latter is true, a certain consequence is inescapable. To facilitate understanding, I shall describe a concrete embodiment of this consequence.

Whatever number of patients Freud indicated, he had a corresponding number of real patients in mind. When he wrote the text of *Gesammelte Werke*, I:435, line 12f., he had decided to assert the usual untruths about recovery and so on, about 16 of the patients. But at this stage he was in doubt as to whether he should also include Johanna and Ralph into his fabrications. However, when Freud wrote line 14ff., he had made the positive decision to include them.

This is a kind of hypothesis that is not formally impossible, but which few researchers would take seriously. I think I am at this stage entitled to conclude that Freud did not have 7 patients in mind when he asserted to have observed this or that in 7 patients, nor 18 patients in his mind, when he made observational postulations about 18 patients.

I do not claim that Freud *never* had any real patients in mind, and *never* applied the detour approach.

§451. But if Freud did not have any particular individuals in mind, whenever he asserted some number of patients about whom he had observed this or that—or when he claimed to have countless successes—it follows that those 11 pa-

tients about whom we are supplied with no information at all, have no counterparts in the empirical world.

It is not relevant that a writer may have numerous legitimate reasons for not supplying concrete information about some or all patients referred to in a paper. If Freud had cited the complete biography of Victor, or another biography of the same evidential power, and had added the summary claim that he was in the possession of 17 additional biographies of a comparable nature, we should believe his words. Anyone who is capable of presenting such excellent evidence, would have no motive for fabricating further secret evidence.

What about the remaining 7 patients? It may safely be assumed that Alice, Beatrice, Florence and Michael really existed, and that Freud actually had them in his mind when talking of them. If Desirée and Elsa are identical with Alice and Beatrice, they will pose no problem. But if they are not, Christina, Desirée and Elsa occupy an intermediary position. They are mentioned only once throughout the third seduction paper. And they appear on the scene exactly when they are *needed* to back up *the deliberate untruth* about *external confirmation*. We know that they never experienced any instance of de-repression; consequently, *everything* postulated of them is deliberately fabricated. It is really a minor point whether the very patients themselves were also fabricated.

§452. We may apply *the criterion of isomorphy* and juxtapose dozens of instances in which events and properties are fabricated ad hoc, exactly because they were momentarily needed to "prove" this or that interpretation.

1. Freud ardently wanted to prove that Dora's asthma was caused by spying upon her parents practicing sexual intercourse. And then he SUDDENLY RECALLED that he had in "many" previous cases verified that asthma was indeed caused by spying upon fathers with sick lungs performing coitus. (Cf. Q-17:1)

2. Freud ardently wanted to prove that Dora masturbated during the latency. It may or may not be true that she wet the bed when she was 7-8. But Freud fabricated ad hoc the empirical generalization that: relapse into bed-wetting after a few dry years is usually caused by masturbation. (Cf. §11.)

3. Either, Freud gave a second thought to the matter and found that he had not in the least proved that Mr. K. was in love with Dora; or someone else (e.g. "Oscar") provided friendly criticism. Therefore Freud added a footnote in which he claimed that Mr. K.'s love was proved by the words he said when he delivered his Christmas present in 1898—words which Freud wisely abstained from quoting. (GW-V:271/SE-VII:108)

4. In October 1900 Dora submitted to psychoanalytic treatment solely because she did not dare to oppose her father's command. She asked for more treatment on 1 April 1902 on her own initiative. She had good reason for choosing that date. She had read about Freud's appointment to be a professor in *Wiener Zeitung* 18.3.1902. She had suffered from a cough attack since the middle of February, and of migraine for weeks. Easter started a few days after the appointment was published, and Dora saw Freud on the very first week day after Easter.

Freud wanted to prove that her visit was meant as a joke, whence she had chosen the date of 1 April. Allegedly, she had no reason to see him at that date. Instead, he fabricated another meeting in October 1902, and also fabricated that she had read about him in the newspaper two weeks prior to that meeting. And allegedly, she had suffered from a cough attack continually for 6 weeks prior to *this* meeting, and—not from *migraine* but from a *facial neuralgia* which, moreover, was triggered off by her reading about Freud.



The reason why Freud chose October for his fabrication might be that the *Wednesday Society* was constituted at that time: an event that is *similar* to the appointment to be a professor.

[Q-452:1]

§453. I shall list only one of the relevant references as regards the last example of Q-452:1, since a full discussion would need much space; since I shall elsewhere document all statements; and since the main points are admitted by the psychoanalysts themselves (cf. James Strachey's footnote and chronological summary on SE-VII:122, 5f.): the October meeting never took place, and in April Dora showed more or less the things Freud attributed to her in October.

A fifth example. Freud claims that the treatment of the rat man lasted for more than 11 months. Since it started on 1 October 1907, it should have terminated in September 1908. However, Mahony (1986:69) found that the treatment did at least not extend beyond 15 July.—The case-notes stop abruptly on 20 January. Considering the fact that Freud found this case and these case-notes immensely important, it is hardly believable that he stopped writing down further notes, if the treatment really proceeded beyond that date.

§454. Something should be said about Christina, Desirée and Elsa, under the assumption that the latter are not identical with Alice and Desirée.

Freud starts out with announcing his aim: to prove the reality of the seduction events that were de-repressed by means of psychoanalytic techniques. And this aim is to be achieved by achieving another aim, viz. to prove that the events have been confirmed by outsiders. And then comes a veritable anticlimax. Freud applies the classical technique of the fabulator who is aware that he has no evidence at all; who fabricates fake-evidence out of thin air; and who is careful to minimize the risk of exposure by avoiding any concrete statement. Please re-read §§283f.

§455. As regards Desirée and Elsa, one explanation is flagrantly the first to suggest itself. Desirée and Elsa allegedly participated in the same group sex act together with the same "male person". Both of them got ill, and both became patients of Freud's. Since their accounts agree, Desirée's account has been verified by Elsa's account.

*But, as we have seen, Freud completely overlooked the following fact. If Desirée's account had been verified by Elsa's concordant account, then Elsa's account had reciprocally been verified by Desirée's concordant account. The dyad Desirée-Elsa must have supplied external verification for TWO patients, not just for ONE.* It is simply not possible that Freud could have made a mistake of this nature, if he had had real individuals in mind, with real names, real hair colour, real nose forms etc.

§456. There is however room for a strange hypothesis, according to which Freud did *not* make a mistake, at least not on the point in question. I shall present the latter in a moment. But I want first to remove any ambiguity as to the power of the different results and hypotheses.

Freud really had less than 18 patients: the evidence for this hypothesis is conclusive.



Freud did not have any particular patients in mind when he claimed to have observed this or that about 7 or 18 patients etc.: the evidence for this hypothesis is not formally conclusive. But it is the habit of science to accept hypotheses, and to consider them sufficiently firmly supported, even if the evidence is significantly weaker than the pattern I have presented. The hypothesis could only be rejected, if certain auxiliary hypotheses be accepted which no researcher is capable of believing in.

Freud had no particular patients in mind corresponding to the 11 about whom we are given no information at all: this hypothesis follows from the preceding one. Hence, it is exactly as strongly verified.

Freud had no particular patients in mind when he talked of Christina, Desirée and Elsa, given that the latter two are not identical with Alice and Beatrice: the probability that this hypothesis is true, is much greater than the probability of its falsity.

As for the last hypothesis that Beatrice was never a patient: the evidence is conspicuously inconclusive.

§457. Now to this hypothesis. I presume that Desirée and Elsa were really identical with Alice and Beatrice. Beatrice was never ill and was never a patient of Freud's. She was merely Alice's friend (or cousin etc.) There was really a scene when Alice and Beatrice and a boy of more or less their age were sitting at a table. And Beatrice heard him ask Alice a question.

Alice eventually developed anxiety attacks. These were probably both causally and temporally unrelated to the just described scene. But Freud constructed the interpretation that her anxiety attacks were really self-reproaches because she had tolerated the boy's hand stroking and leg pressing. Freud fabricated an observation out of this interpretation.

Anyway, Alice was sent to Freud. It is not important whether he treated her for a year or for a month or only saw her once. But when he learned about the antecedent situation, the interpretation spontaneously occurred to him that exactly these three individuals had engaged in group sex when they were very young children. In other words, common presence at the (alleged) infantile event was inferred from no other facts than the common presence at the antecedent event.

§458. Freud felt that his construction would look more plausible if Beatrice had also got ill. Hence, he supplied her with a hysteria, and fabricated that he had treated her.

He attributed anxiety attacks to Beatrice, since this is a symptom that may safely be attributed to any real *or* alleged hysteric. He also constructed an antecedent situation for Beatrice, by transferring the boy's question to her situation. For obvious reasons he provided no information as to who had asked the question. But he retained the truth that the question was not directed toward Beatrice herself.

Alice may well have suffered from a painful sensation in the vagina. Also, Freud may have had an inkling that many readers would be skeptical, if he maintained that such child play had made two adult females ill. Hence, he substituted the natural term with the ambiguous expression "male person".

I have no idea as to whether or not it is an accident that the third seduction paper is ambiguous as to whether or not Desirée and Elsa were identical with Alice and Beatrice.

§459. *If this hypothesis is true, Freud forgot himself and told the truth, by a slip of tongue, as it were. IF Alice (= Desirée) had confirmed Beatrice's (= Elsa's) account, and Beatrice had reciprocally confirmed Alice's account, then Freud would have obtained external confirmation for ONE and NOT FOR TWO out of those cases he really had.*

§460. All aspects of this reconstruction are not equally important. The crucial details are these. First, that Beatrice was neither sick nor a patient of Freud's. Second, that these three individuals were interpreted to have participated in the same group sex act.

The only test of this hypothesis I can imagine, is weak. And regrettably, it is based on a number of auxiliary hypotheses for which there is no sufficient evidence.

If Desirée and Elsa were identical with Alice and Beatrice, we are provided with no information about 13 patients. If Christina was a fictive patient, Freud's sample should consist of 3 females and 1 male. If, in turn, Beatrice was a non-patient, we have finally arrived at the pattern of *two female and one male patients*. And this distribution is the only thing that seems to be testable.

I shall test the distribution "2 + 1" only against one alternative distribution, viz. "3 + 1". A number of plausible arguments may clarify that the former is somewhat more probable.

First, simple proportions, such as 6 males and 12 females, are more prone than complex proportions to invite the suspicion that they are faked. And I think Freud must have known this fact. Consequently, if Freud actually had a small sample with a female proportion of 75% female patients, and he decided to fabricate a greater sample, we should expect him to do one of two things. Either, he should retain the same simple proportion he truly had. Or else, he should claim to have a non-simple proportion. In other words, if a female proportion of 2/3 is observed in the greater non-authentic sample, we may infer that the female proportion of the lesser authentic sample was likewise 2/3.

In the first and third seduction papers the distributions "11 + 2" and "12 + 6" are indicated. Could these figures more easily derive from "2 + 1" than from "3 + 1"? Indeed. In "2 + 1", double the latter number, and reverse the order so that female majority is preserved. Or multiply the original numbers by 6.

Freud invariably applies parsimonious procedures when he distorts or fabricates his observations. Whatever objections may be raised against the above deduction, one is not valid. It is not true that any pairs of numbers could be transformed into other pairs by means of such parsimonious transformations.

In §456 I have listed the degree of verification I claim for various hypotheses. As for the last one, the reader may take it for what it is worth. But I think this hypothesis is sufficiently important to justify its being included in the present report.

## Why Did Freud Retract the Seduction Theory?

*Candidus—what a prophet he is—after examining the stars, proclaimed his wife's goodness to all. When his wife left him for a lover, he took another look at the stars and gave warning to all of her badness.*

Thomas More

§461. A few chapters will, partially or entirely, be devoted to the study of Freud's retraction. But I shall present no exhaustive analysis. This subject is highly complex. Masson (1984) has done much to elucidate it. But even apart from the fact that his main principle is erroneous, additional surprising phenomena may be dug out. For instance, however much Freud had in public retracted the seduction theory, both in 1906 and in particular in 1914, *he still continued to apply the theory in his private practice. As late as in 1922 he delivered an interpretation to Maryse Choisy which is the perfect analogon to the one given to G.de B. in 1897*—as I shall show in a later volume. Freud's thinking was much more stationary and much less developmental, than is usually thought.

§462. Why did Freud retract his theory? A number of explanations may be or have been suggested. The classical explanation was probably the only one in public existence from 1906 to the early 1980s. According to the latter, Freud eventually discovered that his patients had fabricated the seduction events.

It is hard to imagine a hypothesis that could more easily be falsified. We have seen conclusive evidence to the effect that none of the patients ever claimed to have been seduced, while Freud himself had invented the infantile scenes and applied coarse persuasive techniques to induce the patients to accept the interpretations. Freud had even admitted in print that the patients *denied* having been seduced. And he had in print stated *his own* postulation, that the very fact of their denial proves that they have actually been seduced.

He had explained that he was by no means gullible and merely believed what his patients told him. "I should accuse myself of blameworthy credulity if I did not possess more conclusive evidence" (GW-I:418/SE-III:153). He had performed a meticulous test, and had conclusively ruled out the fabrication theory.

§463. Many of the relevant details will be discussed in the subsequent chapter. But when Freud substituted the seduction theory with the fabrication theory, he never tried to explain how the events fabricated by Desirée and Elsa could have been reciprocally verified, although none of these patients were aware of the

events when they started treatment. In §281 I hinted at a construction according to which the condition of independency might not have been satisfied. This construction would allow for retraction, but was evidently not an alternative open to Freud.

It is one of Freud's habitual techniques to blame the patient(s) for defects for which he himself is responsible.

By contrast, one of the recurrent features of the fabulator is this. When he is cornered, he will get himself out of a corner by fabricating a new untruth. The seduction theory begot the fabrication theory. What really happened was that the patients had *masturbated* when they were 2–4. Being unable to take responsibility for their own actions, they transformed their recollections into events of sexual seduction by some other individual.

§464. Logically, this theory is even more curious than the seduction theory. And we may guess that the academic community was more tolerant of the fabrication theory because of *non-scientific* reasons (e.g., it is not offensive to people occupying a high social position). First, childhood masturbation is evidently much less frequent than Freud imagined. Note also that Freud allegedly had in 1905 made an abundance of clinical observations of the prevalence of masturbation around the age of 8 (GW-V:236f./SE-VII:74). Soon afterwards he would develop the concept of *the latency*, the age of 5–11 (GW-VII:205/SE-IX:171) during which there is no external sexual activity. Freud was always prepared to let his own “abundantly verified” empirical generalizations disappear in silence. But if he could be so thoroughly mistaken about masturbation during later childhood, it would surely be unreasonable to take his word on faith as regards masturbation during early childhood.

Besides, an adult abusing a young child may use the mouth, the anus, and other parts of the body. A child would usually use only the genitals. If Michael had manipulated his own penis with his own hand, there would be *some* sense in distorting this action into an adult female having done so with her hand. But *how*—and *why*—did he arrive at the idea that he had been induced to apply his own foot to the vagina of an adult female?

§465. Freud added one more fabrication, viz. that Freud's mistake was really a petty one; from the psychiatric point of view he had not made any mistake at all. It doesn't matter whether the sexual acts be real or imaginary, because the imaginary acts are “psychologically real” to the patient himself.

In 1896 it was said to matter a lot. And isn't it strange that Michael's stiff leg was cured exactly at the moment he started to fabricate about having masturbated with his foot in a vagina? How could it possibly be true that symptoms will disappear when and only when the *authentic* causal event has been dug out? Did Michael not need any insight into the fact that he had masturbated? And did he not need to recall the original masturbation events? Was complete recovery achieved, despite the fact that Michael was still the victim of pathological distortion of reality, deriving from the fact that he could not stand the truth about himself?

The reader may try for himself to apply the fabulation theory to the case of G.de B.—or rather, to the distorted version which Freud might have published. Granted that she had masturbated, and then had transformed her recollection of this activity into the fantasy that her father had practiced fellatio upon her: why did she develop an oral eczema, and why did her eczema disappear at the very moment she “recalled” the fellatio event?

§466. Strangely, few readers have compared the fabulation theory with Freud’s position in the *first* seduction paper:

“Active masturbation must be excluded from my list of the sexual noxae in early childhood which are pathogenic for hysteria. Although it is found so very often side by side with hysteria, this is due to the circumstance that masturbation itself is a much more frequent consequence of abuse or seduction than is supposed.” (GW-I: 382/SE-III:165) [Q-466:1]

§467. I shall now turn to a very important point. There has always been water-tight bulkheads between psychoanalytic theory and psychoanalytic clinical endeavour (observations and interpretations). The theory was never intended to explain or elucidate the observations, and only occasionally the interpretations. Consequently, there are glaring contradictions between the implications of the theory and the semi-theoretical comments upon the real or alleged observations. Contradictions are also found between the theory and the observations.

According to the theory, certain recollections are shielded off from the conscious, because the awareness of them would produce more pain than the conscious could stand. A number of defense mechanisms are applied between the conscious and the unconscious, to prevent the recollections from entering the conscious. The latter are however preserved in the unconscious. Hence, the unconscious is “aware” of the true state of things, while the conscious may suffer from a distorted conception of reality.

§468. Nonetheless, Freud may at the whim of the moment make comments which are altogether incompatible with this “normal” psychoanalytic theory. We may juxtapose all such comments, and try to deduce *what* theory they would entail. Such an endeavour would not be worthwhile, if the comments contradicting the normal theory had little in common but were truly momentous.

But sometimes certain ideas are so recurrently implied, that the most parsimonious explanation is that Freud entertained a rather permanent theory. Such a one is *the theory of the two unconscious minds*. An alternative formulation would be, *the theory of the three hierarchical levels*.

This theory says, in essence, that the psychological equipment of a human being consists of one conscious and two unconscious minds. And the relation between the two unconscious minds is basically the same as the one postulated by the normal theory between the conscious and the unconscious. The lower unconscious applies the same variety of defense mechanisms to prevent certain pieces of knowledge from entering the upper unconscious—either from entering at all, or from entering in an undistorted form.

There are many statements throughout *Gesammelte Werke* where defense



mechanisms are postulated, but which can only be construed as implying that both the original phenomenon, the process of distortion, and the distorted outcome, occur outside the conscious mind.

§469. The complete account must be postponed to a later volume. However, since the fabulation theory is a concrete embodiment of the theory of the two unconscious minds, a brief sketch is appropriate here.

At the start of the psychoanalytic treatment, Michael's lower unconscious was perfectly aware that he had masturbated and had not been seduced. His conscious mind was totally unaware of both phenomena. But some third mind, lower than the conscious and higher than the completely knowledgeable unconscious mind, believed that Michael had applied his foot in the vagina of an adult female in order to produce an orgasm for her. This distortion was evidently produced by means of defense mechanisms. The justification was that it would be too painful for some other unconscious mind to know the full truth.

At the end of the treatment the defense mechanisms between the upper unconscious and the conscious had been removed. And the knowledge hidden in the upper unconscious was given free entrance to the conscious. As a result, the symptoms disappeared.

§470. I shall not suggest that the theory itself is inferior to the normal theory. But a few other points are crucial. It is strange that Freud could for a lifetime entertain a theory without knowing about it. Even great scientists and philosophers have unknowingly entertained implicit theories, which may have been either more or less satisfactory than their usual standard. But Freud claimed to have a unique capacity for observing the authentic state things. (So do his followers, none of whom has detected the implicit theory.) In this respect is it true that neither Freud nor any of his followers have developed a theory of the unconscious.

§471. All in all there are 5 levels: three hierarchically ordered minds, two of which are unconscious. They are separated by two different sets of defense barriers. Now, both defense barriers prevent the conscious from learning the full and non-distorted truth. That is, psychoanalytic treatment could only result in the full truth becoming available to the conscious, if both barriers be removed. Freud explicitly states that the upper defense barrier consumes a non-neglectible part of the energy which would otherwise have been released as sexual orgasm, whence it is logically impossible that any neurotic could experience a full orgasm. We may safely assume that the same thing is true of the lower defense barrier.

We are therefore entitled to ask: how could symptom removal, complete insight, a complete cure, a complete sexual orgasm etc. ever be accomplished, if the lower defense barrier is allowed to remain intact?

How could Michael's lame leg have healed up on account of the de-repression of the distorted version of the upper unconscious, in the absence of de-repression of the authentic state of things into the upper unconscious and the conscious?

§472. My point is not that the theory of the two unconscious minds is per se impossible or implausible—but that it is a quite different theory from the one Freud claimed to have amply verified. It is a recurrent observation that pseudo-



scientists are unaware of the contradictions between their assertions in different contexts. And one more illustration will be supplied in a moment.

One might try out an alternative interpretation of Freud's text. At the beginning of the treatment no part of Michael entertained the idea that he had masturbated with his foot in the vagina of an adult female. This idea was constructed immediately before the latter was released into the conscious. I shall not show why this hypothesis is not compatible with Freud's position. But this solution will not eliminate the three minds and the two defense barriers and it differs more rather than less from the normal theory.

§473. Recall from §§376–390 the biography of Victor. The latter is so constructed, and the relations of the pieces of the jigsaw puzzle are so tight, that the substitution of the proposition that the seduction events had really taken place with the proposition that Victor had merely fabricated the events, would indeed have the consequence that the entire edifice would fall to pieces so that one would have to build an entirely new edifice from scratch.

In the third seduction paper Freud made exactly the same claims about the nature of *his* evidence. But 10 years later he claimed that the very same substitution could most easily be made while each and all the other pieces and features of the edifice remained unchanged.

Let us apply *the method of substitution*. A perfect analogon of Freud would be a constructional engineer who first claimed to have performed a most thorough and protracted investigation, where he had applied the most sensitive and trustworthy instruments. He had thereby disclosed that a certain column within a house is the primary supporting one, so that the roof and all walls would fall to the ground if this column be removed. Ten years later, the very same engineer removed the very same column without giving a second thought to the whole matter. The roof and the walls did not fall to the ground. The engineer did not feel any need of explaining to his students why it did not. Nor did he feel any need of retracting his previous claim about his thorough investigation and trustworthy results.

The engineer was during decades a teacher of constructional technology. He trained his students in applying the very same instruments to the very same kind of tasks. But he never told the students *what* results he had arrived at for the house just mentioned. He made a passing remark to the effect that his mistake was of such a nature that even a highly competent constructional engineer could easily have made it. But he never bothered to teach his students how to apply the instruments in such a way that they did not risk repeating the same kind of mistake.

§474. I shall now present a second hypothesis as to why Freud retracted the seduction theory. This hypothesis is wrong. It was own first guess when I heard about Alice Miller and Jeffrey Masson.

A. Freud had really 18 patients. B. The infantile seduction events were entirely Freud's own fabrication. C. Freud's fabrications were so well-constructed that no superficial analysis would be capable of exposing the fraud. D. Freud had indeed

pointed out uncles and other relatives as the seducers. And he had done this in such a way that these individuals would be able to recognize themselves. E. It is known that 74% of Freud's patients came from the upper classes. Hence, many of those relatives who allegedly had performed acts which might have taken them to jail if exposed, might after some 20–30 years have become a director, a justice of the supreme court, a bishop, a minister, and so on. F. Even if the period for prosecution had expired long ago, persons in such positions would not be over-enthusiastic when they heard rumours about a doctor who told strange things about them. G. In some way or other Freud learned that his reputation and career were endangered. H. He helped himself out of the strait by means of the fabrication that the patients had just fabricated all the seductions.

Apart from item B and H, my expectations were false. Item C was my greatest error. Moreover, no bishop or vice-chancellor of a university would have had any chance of recognizing himself, even if he had actually seduced his niece when she was an infant.

§475. A third erroneous hypothesis is the one suggested by Alice Miller and Jeffrey Masson. Freud's discovery of the reality and causal responsibility of the seduction events recounted by the patients (!) was really correct. But this discovery led to such an enormous hostility that Freud was unable to stand the strain. He retracted his true results because of cowardice and-against his better knowledge. Because of the retraction he became again accepted by the psychiatric profession.

§476. I shall now present a fourth hypothesis that is inherently plausible and probable. But I shall not prove it.

The text of the third seduction paper was first read orally to an audience, At this occasion Freud's persuasive techniques may have been partially successful. Although few listeners could have been convinced, few scepticists might have been able to point out any concrete shortcomings. However, when the article was published in print soon afterwards, many readers must have been struck by the absence of clinical observations. I surmise that these readers repeatedly asked Freud to reveal some of the evidence he allegedly had in abundance and which was "clear as daylight".

Freud had no evidence at all. What was worse: he was incapable of fabricating any fake-evidence. Hence, he was cornered. But when the habitual fabulator is caught, he may help himself out of the corner by inventing a new fabrication. According to the latter, Freud himself was innocent. His *patients* were to blame, because *they* had fabricated the infantile scenes. Since the scenes were purely imaginary, it seemed much less important for others to learn about their exact nature. And Freud escaped exposure.

§477. It is not true that Freud was ever the object of hostility. But it has always been the policy of psychoanalysts to make black appear to be white.

"Freud never stopped feeling isolated, no matter how famous he later became. 'Long after his work had won widespread recognition, Freud continued to act like a man who daily faced the dangerous fire of the enemy' (Puner, 1947:212). [...] As

late as 1914, Freud wrote to Ferenczi that he felt more isolated from the world than ever.'” (Sulloway, 1979:478) [Q 477:1]

But I shall present a modified version of the fourth hypothesis. Perhaps Freud had little trouble with external skeptics.

He might have had much greater problems with his enthusiastic followers, who might wish to learn how to produce de-repression of seduction events—a thing he could not teach them. Nor could he have given a performance, nor supplied any concrete description of *what* had emerged in the consultation room.

Wilhelm Fliess had such a magnetic personality that he did not lose Emma Eckstein's confidence, when he destroyed her life by an incompetent and irresponsible surgical operation. Freud had likewise a personality which allowed him to substitute the presentation of scientific evidence with persuasive stratagems. Again and again he succeeded in evading the most significant and unanswerable objections.

§478. A most extraordinary miracle in the history of psychoanalysis must not go unnoticed. In 1896 Freud claimed to have verified that hysterical symptoms are caused by sexual seduction during infancy. In 1906 and 1914 he claimed that the patients had merely fabricated the seduction events. In 1924 Freud added a footnote to the re-print of the third seduction paper, in which he testified that hysterics behave exactly as he had stated in 1896 (GW-I:440/SE-III:204). This testimony is specifically connected with the claim that hysterics do recount seduction scenes. Evidently, the footnote describes Freud's clinical experience during the subsequent 28 years.

*But from the moment the seduction theory was substituted with the fabrication theory, neither Freud nor any of his followers have encountered even one single patient who fabricated about having been sexually seduced.*

Even if we had no other proof, this miracle alone would suffice to prove that the original 18 patients had not recounted any infantile seduction scenes at all.

§479. The case of Maryse Choisy—to be discussed in a later volume—is not even an apparent exception. It is not claimed that she herself made the interpretation. She did not even believe in Freud's construction, until she had observed some reaction in the alleged seducer, which she mistook for external confirmation.

In the comprehensive and authoritative text by Fenichel (1945), I could find no case of fabrication about infantile seduction. *The Index of Psychoanalytic Writings* lists 95 970 titles until 1973, translations and re-prints etc. excluded. I have looked up the following entries in the register: Fantasy(ies), Fantasy-, Hysteria(s), Hysterical, Seduction. By this approach I shall miss most writings in which fabrication is merely a subsidiary subject. Nonetheless, if it is a universal feature of hysterics that they will fabricate about having been seduced, we should certainly expect to find *some* papers under these entries. I found a total of one paper, Kossak (1913). It will be discussed as a preamble to the analysis of Choisy's cat dream. But even if Kossak's contribution be taken at face value, she merely proves that some children are indeed seduced—not that hysterics fabricate.

—To sum up: the fourth hypothesis is inherently plausible, but it is by no means proved. At the same time, it is the only one of the four ones listed here—which are probably the only four which have ever been suggested—that is not seen at a glance to be false. It should therefore be provisional accepted, until someone manages to formulate a fifth hypothesis that is not flagrantly false.

## Jeffrey Masson's Explanation of Freud's Retraction

*Keine Hexerei, nur Behändigkeit*

Psychotherapeutic patient in  
response to an interpretation

§480. Curiously, many of the interesting facts dug out by Masson flagrantly refute his own conclusions, while they conclusively prove mine. I shall discuss only a few patterns. Not all of them have a direct relation to Freud's retraction. Moreover, I have deemed it preferable to allow for a few repetitions of things already stated elsewhere throughout my report.

First main point. In the third seduction paper Freud explicitly claimed that he had made *clinical observations* of the phenomenon of *de-repression*. All 18 patients had managed to *recall* all repressed events, each of them with all vivid details.

Freud's text is exceedingly vague, but may nevertheless be used to calculate the minimum of de-repressed events he must have listened to. Readers with a liking for this variety of analysis may perform it for themselves. I arrived at an absolute minimum of 48 de-repressed events, and a probable *minimum* of 63 events. An absolute minimum of 30 events would be instances of infantile seduction. The general size of this figure is far from unimportant.

§481. In a letter to Wilhelm Fliess, dated 21 September 1897, that is, exactly 17 months after the third seduction paper was read in public, Freud explained that he had come to doubt his seduction theory. He had previously "*overlooked*" (!) the fact that *de-repression* is a *completely non-existent* phenomenon: whatever has been repressed, will remain so until death. As I have already shown, this statement cannot be taken to mean that Freud had overlooked the possibility that the patients might have fabricated. FREUD HAD "OVERLOOKED" THE FACT THAT NO PATIENT WILL EVER TELL A PSYCHIATRIST THAT HE OR SHE HAS COME TO RECALL A HITHERTO FORGOTTEN EVENT.

We have here one more conclusive proof of the fact that Freud was not telling the truth in his paper. In the September letter he tried to confuse the issue by stating that he had made a mere *mistake* of a *theoretical* nature. The present instance is *isomorphic* with another pattern. Allegedly, Freud had in his early days *observed* that patients will always recover from their symptoms when they *consciously* believe in an interpretation (GW-VII:207/SE-IX:173). Nine years later

(GW–XI:290ff./SE–XVI:280ff.) he presented the claim just mentioned as his former *theoretical belief* that patients *would* recover.

De-repression plays a prominent role in psychoanalytic theory. But a considerable number of circumstances—many but not all of which have already been stated throughout my report—conclusively prove that no psychoanalyst has ever observed this phenomenon.

One might be inclined to conclude that the latter does not exist at all. Interestingly, a genuine instance of de-repression has been observed by a behaviour therapist (Wolpe, 1958:94f.), and a less conclusive instance by another behaviour therapist (Thomson, 1976: 53). De-repression is however an extremely rare phenomenon.

The described pattern is by no means paradoxal. Psychoanalysts have repeatedly made deliberately false assertions (e.g. about symptom removal, “the psychodynamic surplus qualities” = increased enjoyment of sex, life and work)—while non-psychodynamic therapists *have* produced these results.

§482. In a further letter to Fliess, dated 22 December 1897, Freud had again got confidence in the seduction theory. The reason is that he had *observed* three instances of de-repression. At the age of two the patient was brutally deflowered by her father and thereby infected with gonorrhea, “*so that her life was in danger as a result of the loss of blood and vaginitis*” (quoted in Masson, 1984:116, italics added by MS). At the age of three she eavesdropped, and thereby perceived a highly sadistic scene between her parents. At the age of 6–7 months she perceived the fact that her mother was “*almost bleeding to death* as a result of an injury inflicted by her father” (italics added by MS). Freud seems to have had anal intercourse in mind.

§483. The last letter merits many comments; a few of these will be postponed.

Here we are for the first time provided with one very concrete account containing a number of very concrete details, viz. the eavesdropping event. And no one could suggest that there would be reason to exclude this event from a published paper in order to protect the patient’s anonymity.—The other two events are hardly more concrete than the circumstances about Michael’s foot.

Surprisingly, here it is *not* a matter of coitus-like acts against the patient herself, but of genuine coitus. Still more surprisingly, there are very few concrete details about the seduction event. The seduction event is crucially important, while the eavesdropping event is not very significant. Nonetheless, only 30 words are devoted to the former (in the English translation), but 190 words to the latter.

Recall from §480 that Freud dug out at least 30 instances of de-repressed infantile seduction events. Thirty descriptions à 190 words would need 5700 words. The German text of the third seduction paper comprises 9047 words. Hence, such clinical documentation would need some 60% of the space. Freud can hardly have overlooked the fact that it would have made superfluous almost all sentences actually included in the paper. (Since there are no sentences showing that the events were causally responsible for the symptoms, no such sentence would have to be deleted.)

§484. Both Freud and Masson have overlooked that the most crucial aspect is missing from the account: de-repression of the two scenes just mentioned should



have lead to symptom removal. Such information would evidently not have been absent from Freud's letter or from all comparable texts, if Freud had really observed de-repression to be invariably followed by disappearance of symptoms.

The reader may try for himself to reconcile the two events of §481 with the fabulation theory. Why and how would an infant transform her own genital masturbation into fantasies of her father having infected her with gonorrhea and of having watched her mother almost bleeding to death because of coitus per rectum?

There is no reason to discuss the hypothesis that Freud might have fabricated his observations in April 1896, but might 20 months later have obtained genuine observations of exactly the same variety.

§485. He wisely abstained from publishing this new case. Such clinical evidence could hardly fail to evoke requests for logical and factual clarification. How did Freud establish that defloration took place at the age of two rather than, say, at five? How did he achieve de-repression? How did the patient manage to recall what had happened when she was 6–7 months old? The reader may try for himself to figure out how anyone could have *dated* such recollections? How could such a young baby have perceived and conceptualized such abstract events as coitus per rectum, as well as: not merely “bleeding” but “almost bleeding to death”?

Moreover, it might be difficult for Freud to refuse a request from, say, Krafft-Ebing for interviewing the patient. And Freud's letter to Fliess provides informative hints as to *what* an interviewer would observe:

“Have you ever seen a foreign newspaper which went through Russian censorship at the border? Words, entire phrases and sentences obliterated in black, so that the rest becomes unintelligible. Such Russian censorship occurs in psychoses and produces the apparently meaningless deliria.” (Freud, quoted in Masson, 1984:117) [Q-485:1]

It does not seem that Freud used the word “psychosis” in its present sense. But if he had really observed instances of genuine de-repression, or even phenomena which he *believed* to be such instances, it is difficult to believe that Q-485:1 would ever have entered his letter.

§486. My method of textual analysis is evidently related to Q-485:1. I am retrieving the authentic facts behind Freud's letters (and his published writings), by rectifying what Freud has distorted by some kind of “Russian censorship”. My method is certainly not akin to the psychoanalytic standard operation procedure.

The letter mentioned illustrates Freud's method of interpretation:

[OBSERVED FACTS:]

“At the age of sixteen she again sees *her mother bleeding from the uterus (carcinoma)*, which brings about the beginning of her neurosis. The neurosis breaks out one year later when she hears of *an operation for hemorrhoids*. “

[INTERPRETATION:]

“*Can it be doubted* that the father forces the wife into *anal intercourse*? (Freud quoted in Masson, 1984:116f., italics added by MS) [Q-486:1]

Temporal proximity between events may derive from accidental causes; an excellent example was given in §328. But we cannot be sure whether there was at all any temporal proximity, since it is one of Freud's habitual techniques to fabricate temporal relations when he needs them for his proofs (cf. Q-16:1 and elsewhere). Note also that the interpretation is deduced from only two trivial data, and by means of *the principle of similarity*.

Still more importantly: Freud's text seems to say that the patient *recalled and recounted* the events which took place at the ages of 6–7 months, two years, and three years. At the same time Freud's position is clear: the certainty of the infantile events is neither greater nor less than the certainty of *the inference* about anal intercourse in Q-486:1 This can only mean that the infantile events are likewise inferences.

§487. More will be said below about Emma Eckstein. However, she had eventually become a psychoanalyst. In a third letter dated 12 December 1897 Freud communicated to Fliess that: Eckstein had dug out a scene of seduction by the father in one of her patients, although "Eckstein treated her patient deliberately in such a manner as not to give her the slightest hint of what will emerge from the unconscious" (quoted in Masson, 1984:114).

It is a strange belief that such neutral behaviour is possible. We are here confronted with a magnificent illustration of *the illustration of separation*, cf. Q-186:1.

But I am more inclined to think that Freud was aware of applying to other patients the same coarse persuasive technique which he applied to G.de B. Nonetheless, during his entire life Freud postulated that suggestive influence was altogether absent from his consultation room. This problem will be documented and analyzed in a later volume.

§488. It is a remarkable fact that in the letter of 12 December there is no hint as to the three instances of de-repression mentioned in the letter of 22 December. This silence can only mean that all three instances occurred during an interval of 10 days. Also, if the patient had been in treatment for some time, she had given little hint that de-repression was about to happen. Inter alia, there can have been no hint that her chains of associations were about to converge. Recall the fact that in 1924 Freud testified that all hysterics behave exactly as he had stated in 1896.

We may doubt the wisdom of entertaining a strong conviction in any theory after observations made during at most 10 days. This is so much the more true, as the very same theory was previously thought to be supported by a wealth of absolutely conclusive evidence, which had eventually vanished into the air.

§489. As I have said elsewhere, Freud and Fliess had the same habit. Again and again, both of them claimed to be in the possession of an abundance of CLINICAL OBSERVATIONS proving their theory. Here is a selection of clinical observations postulated in Fliess' published writings.

"I have many times observed that the mother's last breath is taken at the exactly same time as her daughter's monthly period sets in, even when the latter had no idea of the

[impending] death. [...] I have been able to predict the dying day by tying it to such a day [menstruation]." (Fliess, quoted in Masson, 1984:96) [Q-489:1]

Just like Freud, Fliess had allegedly *observed* (as testified in the quotations in Masson, 1984:76f.), that many symptoms which had resisted all other treatments given by other doctors, disappeared after the removal of the left middle turbinate bone in the nose, or by applying cocaine or some similar treatment to the nose. Among the symptoms cured in this way are *massive uterine bleeding* as well as *neuralgic stomach pain*. Note that this text was written about 7 years *after* Fliess had made the very opposite real observations about Eckstein.—Allegedly, he had also made *clinical observations* of the fact that *the two symptoms just mentioned are particularly frequent in women who masturbate*.

The only non-trivial difference between Freud and Fliess seems to be that Freud had a more adequate feeling of strategy. He carefully restricted his fabrication to what I shall in a later volume call *semi-explored fields*. One out of several defining characteristic of such a field is that general knowledge is so under-developed that it is very hard or even impossible to disprove even the most erroneous ideas.

§490. I shall add a selection of Fliess' postulations, which are not found in Masson's book. Some of them may throw much light upon the case of Emma Eckstein, which will be discussed in a moment. All my examples are taken from Fliess (1897): *Die Beziehungen zwischen Nase und weibliche Geschlechtsorganen* [The Relations Between the Nose and the Female Sex Organs].

It seems that the nose acts like a safety-valve of menstrual bleeding (p.129). Coitus interruptus or intercourse with a condom may lead to unfortunate change in the inner part of the nose (p.111). The erectile organ of the nose corresponds to clitoris (p.3). Nose bleeding is a kind of substitute menstrual bleeding (p.2). It is "*a well-known fact*" that there are females whose (otherwise normal) menstruation will disappear during pregnancy, but will then be substituted by nose-bleeding every month (p.5) One female experienced a total of one menstrual event, occurring when she was 15. Thereafter, a three-day-period of nose-bleeding started every 29th day. When she became pregnant, nose-bleeding disappeared, but was resumed 6 weeks after birth—but disappeared once more when she became once more pregnant (p.5). An 80-year-old female had alternately uterine bleeding and nose-bleeding at the normal interval (p.4). Nearly all cases of menstrual ailments can be cured by treating the nose (p.9). Fliess has many times verified the fact that: if other parts of the nose than the genital area are treated with cocaine, pathological menstrual pain will *not* be cured (p.12).

§491. Emma Eckstein had been in psychoanalytic treatment for three years when she was operated by Fliess. Masson suggests that she suffered from a menstrual ailment, and that she masturbated. Fliess, who did not bother about the contradiction, claims in a book published 5 years later that

"Women who masturbate are generally dysmenorrheal. They can only be finally cured through an operation on the nose if they truly give up this bad practice." (Fliess, quoted in Masson, 1984:57) [Q-491:1]

Fliess surgically removed her middle nose bone. By this action, based on nothing but a purely speculative theory, Fliess revealed an extreme degree of irresponsibility. In addition, he was an incompetent surgeon. He forgot half a meter of gauze within the patient's nose. During the next months Eckstein was repeatedly on the edge of dying from haemorrhages. Her face was deformed for life, a bad odour emanated from her, and her life became miserable for years.

*Note that she did not lose her confidence in Fliess and Freud after these experiences.*

§492. However, Freud soon acquitted Fliess from what he called "the minimal oversight". Freud claims that this expression and evaluation was made by Breuer. But *the criterion of simple isomorphy* will justify the conclusion that Freud probably fabricated this expression himself and put it into Breuer's mouth. Too many unambiguous instances of this technique are found throughout Freud's writings; we have seen some examples, and we shall see many others elsewhere.

Freud "proved" that Eckstein's bleeding constituted a purely psychogenic hysterical symptom. They would have emerged at the same time, even if no surgical operation had been performed. They were just symbolic expressions of Eckstein's sexual longings; Freud was evidently thinking of *menstruation*.

One of the proofs of the psychogenic theory consists of the fact that Eckstein suffered from *nosebleeds* as a child. Note: during three years of psychoanalytic treatment prior to the operation, Freud had *not* discovered this "significant" fact. But now he was eagerly searching for facts which could be used or misused for acquitting his friend. It is a revealing fact that he did not dig out anything more than such hyper-trivial facts and far-fetched deductions.

§493. *The Canon of Psychoanalytic Methodology* is very much alive here. The comprehensive description of the canon must be postponed to a later volume, but a few words cannot be avoided here.

Note first that not only Freud but also Fliess apply *the principle of similarity*. The erectile organ of the nose corresponds to clitoris exactly because both may swell. And nosebleed corresponds to menstruation.

But Freud first and foremost applies *the psychoanalytic standard operation procedure* (cf. Q-186:1) albeit supported by *the principle of similarity*. Haemorrhages are similar to menstrual bleeding, which is closely associated with the female sex organ, which is closely associated with sexual love. *Consequently*, Eckstein's haemorrhages were *caused* by her love of Fliess.

This argument is analogous to Freud's proofs of Dora's love of Mr. K., some of which were presented in Q-16:1 and §295.

The physical effect of the surgical operation is automatically cut away by means of *the illusion of separation*.

I have not yet defined *the gossip theory of (psychic) disease*. It is the idea that sick people have acquired their symptoms for the purpose of impressing others or obtaining specific gains. Freud explicitly invokes this theory: "Then, in the sanatorium, she became restless during the night BECAUSE OF AN UNCON-

SCIOUS WISH TO ENTICE ME TO GO THERE, and SINCE I DID NOT COME DURING THE NIGHT, SHE RENEWED THE BLEEDINGS, AS AN UNFAILING MEANS OF REAROUSING MY AFFECTION." (Freud, quoted in Masson, 1984:101, capitals added).

I may anticipate that the letters quoted in Masson's book on pp. 100f. provide strong support for my description of Freud's personality in ch. 53.

§494. According to *the postulate of the outgroup*, psychoanalytic theory is true only of the outgroup, that is, of non-psychoanalysts and non-psychoanalyzed individuals. Freud and Fliess are considered objective and rational researchers, free from reality distorting motivation and mechanisms. Not so Eckstein.

Perhaps the most prominent rule is *the principle of prestige*, according to which *an interpretation should always be so constructed or selected that it will increase the prestige of the psychoanalyst and/or decrease the prestige of the patient*.

This principle is usually not applicable to so-called shallow interpretations. But the latter are thought to have no therapeutic effect. They may be used for the purpose of winning the patient's confidence.

The principle is primarily applicable to situations in which the relations between the psychoanalyst and the patient is not very good, from the standpoint of the former. The patient may refuse to believe in flagrantly erroneous interpretations. This rational behaviour is then re-interpreted as "resistance" and "refusal to recover". Or subtle or non-subtle techniques are applied for enraging the patient, whose outbursts are re-labeled "negative transference".

One could not easily imagine any set of interpretations which are more suitable for increasing the prestige of Fliess—and thereby indirectly that of Freud, who shared Fliess's views and was co-responsible for the operation. It may be debated whether the decrease of Eckstein's prestige was an unintended side-effect or a part of the primary goal.

§495. Recall the following words from §482: "so that her life was in danger as a result of the loss of blood". This interpretation may well have been inspired by the case of Eckstein. Cf. also what was said in ch. 3 on the psychological mechanism of *perseveration* as a source of Freud's interpretations. When Eckstein developed the "psychogenic" haemorrhages, she had been in psychoanalytic treatment for some 3 years. In 1905 Freud claimed to have made the clinical observation that: during psychoanalytic treatment new symptoms *cannot* emerge (GW-V:279/SE-VII:116). Hence, both Freud and Fliess were prepared to make public postulations about the invariable nature of their clinical observations, which were strongly discrepant from their actual observations.

§496. Masson is aware that Freud's interpretation of Eckstein's post-operative symptoms as hysterical, was a clear-cut instance of reality-distorting thinking based on irrational motives. But he dogmatically takes for granted that this phenomenon, like the circumstances around the retraction of the seduction theory, were *exceptional* behaviours. He is convinced that the third seduction paper and the letters of 12 and 22 December supply non-distorted and veracious de-



scriptions of completely true and valid empirical results obtained by a most meticulous and objective researcher.

Masson wisely abstains from any attempt at supporting the claim that Freud ever did any piece of acceptable research.

§497. Masson presents it as a fact that Eckstein recounted events of infantile seduction. Listen to his proof. Freud considered Eckstein a hysteric, and Freud entertained the view that hysteria is invariably caused by infantile seduction.

But we have seen ample documentation of the fact that Freud never bothered about contradictions. And it is an instructive fact that Masson completely ignores the following pattern of data. Freud was possessed by the wish of acquitting Fliess. He was vehemently searching for infantile circumstances that could be used or misused for this purpose. He even stressed such trivial facts as nose-bleeds. But he never mentioned infantile seduction. We may safely assume that he would have done so if Eckstein had recounted any such event—or if he himself had inferred it.

Furthermore, *Project for a Scientific Psychology* was written in 1895, and it was not intended for publication. In this writing Eckstein is more extensively discussed. But the greatest approximation to a seduction is the following sentence: "On two occasions when she was a child of eight she had gone into a small shop to buy some sweets, and the shopkeeper had grabbed at her genitals through her clothes" (quoted in Masson, 1984:88).

I am not sure the shopkeeper event was recounted by Eckstein. It is safe to assert exactly this kind of events. Some generations ago most children of both sexes must have been clapped on the buttocks by adults of both sexes for non-sexual reasons.

§498. Masson's proofs of the hostility with which the third seduction paper was met, are found in two places (pp. 6ff. and 134ff.).

He thinks that already the first of these discussions has genuine evidential force. It consists of the following data. 1. After the oral presentation Krafft-Ebing said: "It sounds like a scientific fairy tale." 2. There was no further discussion of the paper at the meeting. 3. *Wiener klinischer Wochenschrift* merely listed the title of Freud's paper but, contrary to its habit, presented no abstract of the content of the paper. And on p. 11 Masson insinuates that *anti-Semitism* was a reason for the silence of this periodical.

It is improbable that Krafft-Ebing or any other member of the auditory realized that the whole paper is a fraud. But most listeners must have been struck with the absence of any kind of evidence. Freud had dogmatically asserted that he was in the possession of a wealth of *secret* evidence. He had accused the audience of hostility because it was not convinced by the presented paper. What then was there to be debated? It may certainly be argued that the silence of the periodical derived from forbearance with a young doctor who had flopped.

In §§200, 365 and 477 I noted the fact that Freud was never isolated, but always felt isolated. But according to Masson, Freud became isolated *because of* the seduction paper—while "giving up his 'erroneous' view allowed Freud to



participate again in a medical society that had earlier ostracized him" (p. 10, 12).

§499. In the third seduction paper Freud made it absolutely clear that he was perfectly aware of the fact that many children have been seduced, but have nonetheless never developed any hysterical symptoms. He carefully explained that infantile seduction is a *necessary* but not a *sufficient* cause of hysteria.

In *Drei Abhandlungen zur Sexualtheorie* (1905) Freud explained his reason for abandoning the seduction theory. He had meanwhile learned something which he did *not know* in 1896, and which was an "*unexpected finding*", viz. that there exist individuals who have been seduced during childhood or infancy, but have nevertheless not become hysterical. (Masson, 1984:129f., drew my attention to this passage.)

Masson supplies strong though not entirely conclusive evidence for the following hypothesis. The unexpected findings did *not* consist of any further *clinical observations from Freud's own practice*. They were instead taken from Havelock Ellis' *Studies in the Psychology of Sex*. But Freud had *not* read this book. He had merely read an account of it written by another writer, Iwan Bloch. But Bloch had neither read Ellis' book. He had merely read a review of it written by Ludwig Hopf. This is certainly a curious basis for retracting a theory.

§500. Earlier in 1905 Freud had in another paper (GW-V:149-159/SE-VII:270-279) explained the retraction by the following arguments. a. his material in 1896 was "still scanty". b. "it happened by chance to include a disproportionately large number of" cases agreeing with the theory. c. Freud was "AT THAT PERIOD" unable to distinguish between true accounts and fabrications.

"A disproportionately large number" is a curious formulation about 100%. And if Masson's hypothesis is true, the proportion was still 100% when Freud wrote these words. Indeed, the proportion was still 100% in 1905, according to *Drei Abhandlungen*:

"I cannot admit that in my paper "The Aetiology of Hysteria" (1896) I exaggerated the frequency or importance of that influence" [= infantile seduction]. (GW-V:91/SE-VII:190) [Q-500:1]

Moreover, a series of 18 *identical* cases is certainly not "scanty" material. And in Freud's private letters a number of additional cases of equally conclusive evidential power are described.

The third point implies that Freud meanwhile learned to distinguish between authentic and fabricated accounts. But *how* had he learned this if he had meanwhile solely encountered authentic accounts? Besides, Freud had in 1896 testified that he had accepted the historical authenticity of the patient's accounts only after having ruled out the fabrication hypothesis by means of a meticulous test. If this test had turned out to be altogether fallacious, wouldn't it have been wise to be a little more modest in 1905?

§501. According to Masson (1984:130), the first *fully articulated* account of the substitution of the seduction theory with the fabrication theory was published

in 1914 in *On the History of the Psychoanalytic Movement*. This is a most unusual place for presenting a fundamental theoretical innovation.

In this paper we are told that fantasies of having been seduced "were intended to cover up the autoerotic activity of the first years of childhood (GW-X:56/SE-XIV:17f.). Freud is thinking of masturbation rather than of such things as pleasure at defecation. The difficulties of the new theory have been discussed in §§464ff. It cannot be repeated too often that all attempts at improving psychoanalytic theory do not remedy the original defect(s), but merely add further shortcomings that are usually even more devastating.

I have already in Q-115:1 mentioned Masson's thorough misrepresentation of the content of the third seduction paper—a fact that supplies strong support for the hypothesis developed in §§225ff., that Masson's and Miller's motive is to provide job facilities at incest clinics for underemployed psychoanalysts.

§502. As was said in §479 I have probably missed some psychoanalytic papers on infantile seduction. Masson mentions a few of Sandor Ferenczi's latest or posthumous papers, together with the diary of his patients. One paper from 1932 is quoted in toto, while excerpts are given from others. Contrary to Masson's strong assertion, none of these texts contain any observation of infantile seduction. Ferenczi merely applied the principle of similarity together with the psychoanalytic standard operation procedure to *infer* such events, and Masson merely applies the same techniques to fabricate that Ferenczi described observations. Little more is illustrated by these data than the application of *the illusion of separation*. It is a well-known fact that it is much more easy to influence the dreams of another person, than his or her awake life. But Masson talks of "*Ferenczi's tenacious insistence on the truth of what his patients told him.*"

§503. Throughout his book Masson cites example after example of Freud being extremely narrow-minded, prejudiced, mean, blind of the most conspicuous facts, lacking in the most elementary knowledge of human nature, prone to make the most extreme distortions of reality on the basis of wishfulfillment, having a very poor capacity for clinical observation and an equally poor insight into methodology. It seems perplexing that anyone having such traits would ever be capable of performing genuine research. In fact, Masson wisely abstains from making any attempt at proving that any of Freud's statements had any empirical or scientific foundation. Masson merely disseminates *the dogmatic assertion* that all Freud's statements about having observed infantile seduction events, constitute descriptions of genuine empirical results, obtained by scientifically appropriate methods by an honest, competent and responsible researcher.

## Alexander Schusdek's Explanation of Freud's Retraction

*It would be indefensible simply to nullify the immense pool of psychoanalytic data, just because of meddlesome purism.*

Bert Westerlundh

*To anyone trying to question Freud's descriptions and analyses, the fundamental problem is that there is generally next to no possibility of disclosing what data Freud actually observed during his discourses with his patients.*

Bert Westerlundh

§504. The main difference between Masson and Schusdek is that the latter takes for granted that the fabulation theory is true. He has overlooked Freud's explicit admission that he himself had invented the seduction interpretations; that he had used persuasive techniques to induce the patients to accept these interpretations; and that the patients had shown strong resistance and had denied having ever been sexually seduced.

In the beginning of his career the only goals of psychoanalytic treatment were symptom removal and guarantee against relapse. And Freud entertained no more than a mere hope that the latter goal could be achieved at all. Consequently, the treatment would be finished as soon as all pathogenic recollections had been de-repressed. But Schusdek supplies a magnificent anachronism:

"The 'hundred hours or more' refers to total time in treatment rather than to the minimum necessary to elicit these scenes." (Schusdek, 1966:162) [Q-504:1]

§505. Schusdek also thinks it possible to estimate the frequency with which cases of infantile seduction turned up. But we have already seen in ch. 20 that Freud's postulations about *numbers* of cases of which this or that were true, had no counter-part in his clinical observations.

A passing remark. I have in ch. 21 suggested that Freud may have had no more than three real patients in mind, when he claimed to have cured 18 patients. I do not conclude from this fact that Freud had only three hysterical patients in treatment during the relevant period. But quite a few psychoanalytic commentators have advanced the idea that Freud got an increasingly more favourable reputation as an expert of hysteria. Hence, the number of hysterical patients asking for treatment were growing.

However, in his letter to Fliess of 25 April 1900 Freud writes:

"I have not succeeded in getting a new patient. The last one who did not show up was a twelve-year-old boy, a grandson of the painter Alt. Although we had agreed on his coming weeks ago, he supposedly fell ill on the day he was to start." (Freud, 1985:411) [Q-505:1]

No new patient for weeks: this statement does not seem to indicate a large practice.

§506. Schusdek suggests that the seduction theory was closely connected with the *pressure technique*.

"One must consider the possibility that the seduction theory was to some degree an artifact of the 'pressure technique'. The patient was told that something would occur to him when his forehead was pressed. He understood that his recovery depended on it. The procedure was repeated as often as necessary. Associations which were not deemed adequate to explain the symptoms were rejected. The patients may have received some from the therapist." (Schusdek, 1966:163) [Q-506:1]

But allegedly, Freud became suspicious because of the frequency with which fathers were accused of sexual abuse. And when he abandoned the pressure technique, his patients ceased to fabricate about seduction.

The entire second book of the present report contains a wealth of empirical data which refute Schusdek's theory. The reader may recall some of these himself.

§507. Schusdek takes at face value the following quotation from 1932:

"In the period in which the main interest was directed to discovering infantile sexual traumas, almost all my women patients told me that they had been seduced by their fathers." (GW-XV:128/SE-XXII:120) [Q-507:1]

If this were true, it is impossible to comprehend how Freud had at that time arrived at the theory that 1. hysteria is caused by passive sexual seduction; 2. compulsive neurosis is primarily caused by active infantile seduction, that is, by sexual seduction of another child during childhood; 3. anxiety neurosis is caused by deficient sexual orgasm due to sexual abstinence, the practicing of coitus interruptus etc.; 4. neurasthenia is caused by masturbation.

Also, we have seen that at least 7 out of the "12" "hysterical" females of the sample of April 1896 had solely been seduced by another child. We are also told that some (pluralis) of the patients had solely experienced assaults produced by strangers. Formally speaking, Freud's text might be construed as indicating that only boys had been seduced by strangers. But in actual practice girls are under much greater risk of having such experiences.

Unless the population of Freud's patients were extremely atypical, hysterics formed a minority. A reasonable estimation is therefore that, out of 100 successfully treated females, a *maximum* of some 3-5% would recount a seduction event involving their father.

(I have generously allowed for the interpretation that servants had solely seduced boys.)

§508. From Freud's letter to Fliess of 21 September 1897 Schusdek takes the following four reasons for the abandonment of the seduction theory:

- “1) continued disappointments, inability to bring the analyses to a real conclusion and the ‘possibility of explaining my partial successes in other familiar ways’;
- 2) the frequency with which blame was laid on perverse acts by the father. Freud could not believe that perverted acts against children were so common;
- 3) there is no ‘indication of reality’ in the unconscious. It is impossible to distinguish between truth and emotionally charged fiction. ‘This leaves open the possible explanation that sexual phantasy regularly makes use of the theme of the parents’;
- 4) these memories do not break through even in deep-reaching psychoses and confused states of delirium.” (Schusdek, 1966:161) [Q-508:1]

Many of the appropriate comments to these statements have already been made elsewhere. But it is a most conspicuous fact that one circumstance is *not* included in this list: the patients themselves deny having been seduced.

If Freud had previously told the truth, it is simply not possible that he had not regularly succeeded in bringing the analyses to a real conclusion; that the seducers were in the majority of the cases some person(s) other than the father; and that the memories can be made to break through by means of psychoanalytic intervention.

I shall disregard the question what conclusion a genuine scientist would be entitled to draw. Instead, I shall ask what conclusion we might expect Freud, as a psychological mind, to draw. I suggest the following. If he really had *many* successful cases in April 1896, then only *many* failures between April 1896 and September 1897 could have lead him to doubt the seduction theory. Even 50 consecutive failures would much more easily have lead him to the view that there are two very different kinds of hysterics.

If he had encountered a sufficiently sizable number of consecutive failures, it is hard to imagine that 1–2 cases encountered in December 1897 would in 10 days completely alleviate any doubt in the seduction theory.

§509. We should not expect Freud to do what any genuine researcher would have done. Noting the fact that each of 18 consecutive cases had lost all their symptoms exactly at the moment they started to fabulate about having been seduced, a scientist would have set out to search for an explanation of such a remarkable empirical phenomenon. But we should neither expect Freud to be altogether oblivious and disinterested in this phenomenon.

And if Freud found it necessary to acquit *fathers*, why did he at the same time acquit all other seducers? Why did he not try out the possibility that the patients might have made a mistake as to the identity of the seducer?

The reader will note the similarity between some of the reasons stated in Q-508:1 and the objections anticipated from others in the third seduction paper. It is a revealing fact that these objections were previously taken to prove the hostility and prejudices of the audience.

§510. But in the letter a few more reasons are stated, which may or may not have been deleted from the edition available to Schusdek:

- a. “The running away of people who for a period of time had been most gripped [by analysis];”
- b. “The surprise that in all cases, the *father*, not excluding my own, had to be accused of being perverse.” (Freud, 1985:264) [Q-510:1]

The first of these reasons is most revealing. *At what point did they run away?* If it happened before they were cured, before any recollection or conviction about seduction had occurred, as well as before any seduction interpretation had been presented, it is difficult to grasp why dropping out should influence Freud's theory. The same thing is true, if they had terminated after de-repression which was immediately followed by symptom removal. Hence, the consequence is inescapable that: either, they had been induced to believe in a seduction interpretation, and nonetheless did not remain in treatment; or they had experienced de-repression which was *not* followed by symptom removal. Whichever alternative we chose, Freud had not told the truth in the third seduction paper.

The first reason may easily be interpreted as meaning: *I do no longer believe in the seduction theory, because my clinical practice (and hence my economy) will be endangered, if I proceed to deliver seduction interpretations.*

This dynamic is actually a palpable reason behind many changes in psychoanalytic theory, as I suggested in ch. 10.

§511. If Freud had had the kind of evidence corresponding to his claims, it is difficult to imagine that he would have rejected the theory on the basis of a mere *feeling*. In addition, the idea of the frequency of perverse fathers could not have been new to him in September 1897.

What does Freud mean by referring to his own father? Did he consider himself a hysteric?

Freud's personality will be discussed in a later volume. However, due attention should be given to the possibility that Freud *started* with the fantasy that he himself had been sexually seduced, and then projected this idea upon his patients. He may have ceased to attribute seduction experiences upon the patients, simply because he ceased to attribute them to himself.

Why did he acquit his own father? Possibly because, *when* he wrote the letter to Fliess, the first anniversary of his father's death was about to occur. The letter is dated 21 September, the father died on 23 October.

At the present point this is no more than a hypothesis. However, there are many instances of Freud having based his theoretical positions and clinical interpretations upon his own subjective experiences. In §431 we noted that: until Freud had had such an experience himself, he was unable to imagine that there could be any individual who would not perceive his father's death as a trivial event. After Freud's own father had died, he was unable to imagine that there could be any individual who would indeed perceive such an event as trivial.



## Theories Asserted by Freud But Not Intended to Be Accepted by the Reader

*When a man takes your coat, let him have your shirt as well.*

Luke 6:29

§512. Again and again we have seen Freud asserting theories and results, which he wanted his colleagues and others to accept, although they had no foundation whatever in his own clinical experience. Somewhat surprisingly, *Gesammelte Werke* also contains a non-neglectible number of theoretical and empirical assertions which are definitely not intended to be believed by the reader. I venture to present Freud's motive or reason already at this stage. These theories and alleged results are intended as a kind of persuasive technique: their aim is to reduce the discrepancy between the positions of Freud himself and some of his opponents.

As our first example we may choose Freud's *doctrine of the priority of somatic etiological factors over psychic causes in hysteria*:

"In this connection we must recall the question which has so often been raised, whether the symptoms of hysteria are of psychical or of somatic origin, or whether, if the former is granted, they are necessarily *all* of them psychically determined. [...] As far as I can see, every hysterical symptom involves the participation of *both* sides. It cannot occur without the presence of a certain degree of *somatic compliance* offered by some normal or pathological process in or connected with one of the bodily organs. And it cannot occur more than once—and the capacity for repeating itself is one of the characteristics of a hysterical symptom—unless it has a psychical significance, a *meaning*. The hysterical symptom does not carry this meaning with it, but the meaning is lent to it, soldered to it, as it were; and in every instance the meaning can be a different one, according to the nature of the suppressed thoughts which are struggling for expression."

[...] "In my estimation a single unconscious mental process or phantasy will scarcely ever suffice for the production of a symptom." (GW-V:200, 206/SE-VII:40f., 47) [Q-512:1]

§513. Let us list the main propositions of Q-512:1.

- A. *One psychic factor will hardly ever suffice to produce a hysterical symptom.*
- B. *One somatic factor will, even in the absence of any psychic factor, be capable of producing a hysterical symptom.*
- C. *At the first occurrence of a symptom, any psychic factor is usually absent.*
- D. *Any number of somatic factors will not be capable of producing more than one occurrence of a hysterical symptom.*
- E. *A hysterical symptom can only be maintained by psychic factors. But it need not be*

*maintained by the same psychic factor. A series of consecutive psychic factors may well produce this effect.* [Q-513:1]

This theory is counter-intuitive, and would not easily occur to anyone aiming at explaining genuine empirical observations.

However this may be, the doctrine of somatic priority can hardly be reconciled with the psychoanalytic core theory. Let us try; I take the liberty of adding a definite age indication to Freud's biography.

When Michael was 3 years old, a maid trained him to masturbate upon her with his leg. Even if she had spanked him into obedience, and even if the whole act was extremely repulsive to him, he would never have got the hysterical symptom of a stiff leg, unless the purely somatogenic symptom of a stiff leg had by accident offered itself, as it were. But if Michael had not been sexually seduced, the stiff leg would immediately have healed up. The psychic dynamic captured the somatic symptom and prevented spontaneous recovery.

If, on the other hand, the somatogenic symptom had been a stiff *arm*, or a *feeble* lame leg, no hysterical symptom would have emerged—because of the rule that the symptom must be *similar* to the original causal event.

§514. Evidently, Freud cannot be saved by the fact that he retracted the seduction theory. And all his interpretations—even in the case-study of Dora, from which Q-512:1 is taken—contradict the doctrine in the most flagrant way.

We were told that the spying event *caused* Dora's asthma (cf. Q-17:1)—not, that one asthmatic attack occurred because of some somatic factor, whereafter the unconscious psychic dynamics "took over". And listen to this account:

"One day she complained of a professedly new symptom, which consisted of piercing gastric pains. 'Whom are you copying now?' I asked her, and found I had hit the mark." (GW-V:197/SE-VII:38) [Q-514:1]

Recall from §421 Freud's postulation that new symptoms *cannot* emerge during psychoanalytic treatment. Was the word "angeblich" = "professedly" added to reconcile this contradiction?

But the crucial point is this. The very moment Freud was told about Dora's symptom, he spontaneously asserted that the symptom was the sole result of a *psychic* factor. And he went on to describe the latter.

§515. Conspicuously, Freud did not adhere to the doctrine of somatic priority. There are also other theories to which he did not adhere. For instance, Freud never believed that psychoanalytic treatment will lead to de-repression or recovery. But he certainly wanted others to believe in the latter two things.

However, no sensible motive could be suggested as to why Freud should want his colleagues, the lay community, or prospective patients, to believe in the somatic doctrine. A little reflexion will certify that he could only have had *one* kind of strategic motive, viz. to soothe certain potential opponents who strongly adhered to somatogenic theories of hysteria.

§516. The same explanation immediately suggests itself as regards another qualification in the case-study of Dora:

"It would be wrong to suppose that dreams and their interpretation occupy such a prominent position in all psycho-analyses as they do in this example." [= in this case-study] (GW-V:168/SE-VII:II) [Q-516:1]

The case-study of Dora contains a total of two (= 2) dreams—a most modest number for a period of 11 weeks. In addition, these dreams are hardly used to prove anything at all, except interpretations which had already been proved by other means.

Freud claimed to have proved 5 encompassing propositions about Dora, viz. that all her symptoms were purely psychogenic conversion symptoms; that she was in love with Mr. K.; that she was homosexual and attracted to Mrs. K.; that she had masturbated during the latency; and that she wished to sleep with her father.—In Q-16:1, Q-17:1 and §§295 and 493 we have already seen proofs not involving dreams, as regards her love of Mr. K., her childhood masturbation, and the psychogenic etiology of her asthma, cough attacks, and dragging leg.

§517. Our third example has a direct bearing upon the seduction theory:

"Every case of hysteria exhibits symptoms which are determined, not by infantile but by later, often by recent, experiences. Other symptoms, it is true, go back to the very earliest experiences." (GW-I:451/SE-214) [Q-517:1]

But 15 pages previously Freud had made an altogether incompatible claim:

"[...] my assertion that the aetiology of hysteria lies in sexual life [...], you will find that it is supported by the fact that in some eighteen cases of hysteria I have been able to discover this connection in every single symptom, and, where the circumstances allowed, to confirm it by therapeutic success." (GW-I:435/SE-III:199) [Q-517:2]

This is one of the contradictions in the seduction juxtaposition, Q-14:1. Every hysteric has symptoms (pluralis!) without any infantile root: *non-IR symptoms*. Consequently, each of the 18 patients had at least two symptoms which were not caused by infantile seduction, nor by any other infantile event nor childhood experience. Nonetheless, Freud dug out the infantile events that had caused each and all symptoms of each and all 18 patients.

The contradiction cannot be resolved by means of the following suggestion. In Q-517:2 Freud states that all symptoms have a *sexual* etiology, which may or may not likewise be infantile, while he in Q-517:1 states that some symptoms do not have an etiology that is *both infantile and sexual*. Q-517:2 is embedded in preceding and subsequent sections which unambiguously rule this interpretation out.

§518. I have already noted the strange fact that we are told nothing about what kinds of symptoms could be non-IR symptoms. No non-IR symptom seems to have been listed in the patient biographies in §243.

Another question throws much light upon Freud's deficient reality feeling deriving from the fact that he was fabricating: *How must non-IR symptoms be cured?*

§519. Allegedly, all patients had at least two non-IR symptoms, and at least

one IR symptom; hence, at least three symptoms. Some of the patients had at least three IR symptoms, hence, at least 5 symptoms.

Now, what happened around the beginning and the termination of the illness? Did a minimum of three symptoms emerge exactly at the same time? And did a minimum of three symptoms disappear exactly at the same time?

But if Freud's text is not taken literally, we are certainly entitled to have some information as to the length of the interval during which any patient had less than three symptoms.

It would be a strange theory that the disappearance of non-IR symptoms should somehow be dependent upon the de-repression of infantile events and vice versa.

And the theory will not be more adequate if restricted to the IR symptoms. Some of the patients had at least three IR symptoms, deriving from three different infantile events. Did all three events become conscious exactly at the same time (during the same consultation)? Or one at a time?

Under the latter alternative: did any symptom disappear suddenly, when the causal event associated with this symptom had become de-repressed? Or did all symptoms disappear at one stroke, but not until *all* causal events had become de-repressed? Or were all symptoms gradually weakened, when some and only some of the pathogenic events had been recalled? A number of further patterns may be suggested.

Such details would not have been missing if Freud had not fabricated his observations.

§520. *The tuberculosis model of anxiety neurosis* was presented in Q-428:1 and analyzed in ch. 20. This is one more theory not intended to be taken seriously.

In vol. II Freud's *first* paper on the anxiety neurosis will be extensively analyzed. At the present moment shall merely anticipate a few results.

Both papers were written and published within 6 months. And in the second paper Freud makes it explicit that he is defending the very same position he took in the first paper, thereby merely giving a more detailed presentation of the very same theory.

Nonetheless, altogether different and incompatible positions and theories are asserted in both papers. If Freud told the truth in the first paper, he had made a wealth of clinical observations to the effect that the causal significance of hereditary factors is negligible, while precipitating causes are very rare occurrences.

§521. Freud states that neurasthenia is the result of masturbation, while anxiety neurosis is the result of sexual abstinence or the practicing of coitus interruptus. However, we are told that coitus interruptus will almost invariably be harmful to *one* of the participants, but hardly ever to *both*. If the male tries to *prolong* the act so as to provide the female with an orgasm, his *prolonging behaviour* will be harmful to himself. *If he does not prolong the act, coitus interruptus will hardly ever be harmful to himself (!)* But it will be so to the female who will miss the orgasm. (GW-I:326f./SE-III:100f.)

It is absolutely impossible that Freud could have made any observations even

remotely similar to the postulated ones. There is no little variability as to the duration of completed acts. The shortening of the latter with a few seconds cannot have any uniform consequence for the female. And quite different aspects of the act are much more uncomfortable to the male.

§522. Freud claims to have observed an abundance of cases certifying the following generalization. A male may first masturbate and, hence, get a neurasthenia. Then he may fall in love with a female, cease to masturbate, but not sleep with her prior to the wedding. When masturbation is substituted with abstinence, the neurasthenia will invariably heal up, and will immediately be substituted with an anxiety neurosis.

Such a perfect correlation leaves no room for any hereditary or precipitating etiological factors.

As if this were not enough, we are even told that: in cases in which sexuality is released through nocturnal emissions, an anxiety neurosis will never emerge (GW-I:336/SE-III:110).

There must be extremely few non-old males who practice neither intercourse nor masturbation and, nonetheless, have no nocturnal emissions. Evidently, it did not occur to Freud that the present observation claim necessarily entails that an anxiety neurosis may hardly ever befall a male—a consequence flagrantly contradicting other equally strongly asserted observation claims of his.

Common sense alone will make it clear that Freud is fabricating. He is fabricating ad hoc a host of empirical generalizations allegedly obtained in his clinical practice. Flagrantly, Freud has no real patients in mind. He did not even have the minimum of reality feeling needed for sensing what kinds of observational patterns *could at all* be found in the empirical world.

§523. I shall defend Freud against two recurrent criticisms. First, it is frequently suggested that Freud made the bulk of his observations upon sexually frustrated Viennese over-class females, and then made the mistake of projecting the obtained results to all members of mankind. Second, Freud is sometimes accused of being himself obsessed by sex, and of having projected his own personal experiences upon his patients.

No one could have made the postulated observations upon *any* set of really existing patients, however non-representative the latter may be. Moreover, if Freud had actually used his own subjective experiences of his own psychological reactions as a basis for psychoanalytic theory, the latter would have been enormously much better than it is.

(But I surmise that in that case psychoanalysis would not have become generally accepted. A theory that is correct to 20% or 80% will usually be evaluated on the basis of its actual merits and shortcomings. It will soon be improved or substituted with a better theory. A theory that is correct to 0% may escape any evaluation.)

§524. The reader must wait for some of the evidential support of my claims until he has read vol. II. Anyway, *the tuberculosis model of the anxiety neurosis was produced and asserted solely for persuasive reasons.*



In January 1895 Freud had asserted that anxiety neurosis is invariably the effect of deficient sexual orgasm. In March Löwenfeld (1895) had objected that Freud had overlooked certain facts. At this stage of the debate Löwenfeld had taken *the more comprehensive position*. Thereby he seemed to be *more qualified* of judging what conclusions are really justified.

In July Freud published an answer. He feigned to have merely spelled out in more detail the position he had stated in the first paper. But now he "proved" himself to be *even more qualified* because he had an *even more comprehensive position*, which involved a whole system of variables.

Two facts are indisputable. First, it is *a standard technique of debate* to prove one's greater competence by *untruthfully* claiming to entertain a more comprehensive position. Second, Freud simply *could not* have asserted the tuberculosis model because he considered it true. Nowhere in Freud's writings can any concrete interpretation of any (veracious or fabricated) specific patient reaction be found, which is not based upon the assumption that psychoanalytic energy transformation, defense mechanisms and symbolism are exhaustive causal factors.

§525. Soon after the publishing of the 5 papers on anxiety neurosis and the seduction theory, Freud trained Felix Gattel in psychoanalytic technique. An entire book in vol. II will be devoted to the analysis of Gattel's (1898) test of Freud's theories upon 100 non-selected cases. I shall quote, verbatim and in English translation, all biographies of hysterics, together with the case-notes of a complete psychoanalytic treatment performed by Gattel in 1897. This is almost certainly the first treatment ever described in print.

As I said in ch. 17, Gattel is eager to explain away uncomfortable facts. But he cannot be suspected of having fabricated or deliberately distorted any observations.

In Q-362:1 I quoted three patient biographies. Gattel thinks that these three patterns of observations prove that the symptoms of all three patients were *caused* by masturbation.

I must also repeat the most crucial circumstance. Freud himself made exactly the same verdicts, and was very much impressed by the evidential power of Gattel's research. And when Gattel published his book without Freud's name appearing on the front-page as a co-author, Freud got angry and accused Gattel of having **PLAGIARIZED** Freud's theories.

I have already mentioned the fact that I myself, by means of the method of textual analysis applied to Freud's published texts, extracted the information that Freud's secret data actually consists of such extremely few, hyper-shallow and hyper-trivial observations as the ones illustrated in Q-362:1. It is provable that I could not have known about Gattel's research when I tried to publish my results.

§526. One aspect is crucial. Gattel found 59 anxiety neurotics. And for all of these he dug out a total of 6 relatives (while he overlooked one more individual) who had *any* kind of psychic disturbance. In all but one of the cases he detected, the disease of the relative most probably had no hereditary relation to the patient's own ailment. Since Gattel nonetheless thinks that his data support Freud's



theories, it is not a far-fetched guess that Freud somehow taught Gattel not to take the tuberculosis model seriously.

The same may be true of the claim about nocturnal emissions as a protection against having an anxiety neurosis. Gattel found abstinent non-masturbating males of the ages: 21, 22, 24, 24, 27, 28, 31, 49. He is completely silent as to whether or not these males had any nocturnal emissions. It would be a sensational discovery if they had not. But if they had, they constitute clear-cut counter-instances of Freud's theory.

§527. Again and again the superiority of the *extensional approach* is apparent. If all examples discussed throughout the present chapter are juxtaposed, and if we ask what is the common denominator, the answer will almost suggest itself. All theories are flagrantly aimed at reducing the difference between the position really entertained by Freud, and those of some of his more influential opponents.

Perhaps the last example may be conceived of as an exception. Freud does to some extent reduce the discrepancy between himself and Löwenfeld. But he also implies that there is a real discrepancy—only, he differs in the opposite direction of the one imagined by Löwenfeld.

It is no uncommon academic policy to try to mitigate the discrepancy between one's actual view and some prevalent ideas. However, the extent to which Freud did so, involving no less than the construction of entire theories, seems unequalled,—except among *acknowledged* pseudo-scientists.

Somehow, Freud was capable of emitting the appropriate signals to his own proponents. They have been keenly aware of what theories were solely intended to confuse the issue in debates with outsiders.

## Miscellaneous Aspects Related to the Seduction Theory

*Ach Larifari, Herr Pfarrer! wüssten Sie was Gescheiters,  
so würden Sies vorbringen.*

Bettina von Arnim

§528. In the present chapter I shall hastily comment upon many aspects, some of which may be of secondary importance.

The problem of the therapeutic effect of psychoanalysis will be commented upon again and again in many chapters in several volumes. Ch. 14 was primarily devoted to the analysis of therapeutic claims. One aspect should be mentioned now.

During the 19th century, most psychotherapists showed such a diminutive recovery rate that it may be questioned whether their patients would have been worse off without any treatment at all.

The case described in ch. 2 of Charcot (1987) should not be mentioned as a clear-cut example of unsuccessful treatment. Instead, it should be seen as an excellent presentation of the relevant circumstances. Hence, the reader may form his own judgement. I shall not discuss this case, but merely state my own evaluation. The patient's epileptic attacks became somewhat less frequent after the treatment. But they had never any great frequency, while the frequency was previously markedly irregular. Consequently, it is by no means impossible, but far from certain, that the treatment was responsible for the decrease of frequency.

(Additional aspects of Charcot's book will be discussed below.)

§529. I do not pretend to be highly familiar with psychiatric writings throughout the 19th century. Nonetheless, the only therapist known to me who has published any genuinely convincing examples of therapeutic benefit, is Albert Moll (1889; 1895). We do not know the size of the totalsample from which the successful cases were selected. But 16 cases are described in some detail in 1895:263–269. Symptom removal occurred in close proximity to the hypnotic intervention. I cannot recall having encountered any comparable case in the entire psychoanalytically oriented literature.

§530. No therapist is known to me, apart from Freud and his followers, who has claimed to have produced complete recovery by a specific therapeutic method, where neither recovery nor the patients existed, nor even a therapeutic procedure which was at least remotely similar to the postulated one.

On the other hand, throughout both the 19th and the 20th centuries, numerous therapists made exaggerated claims about therapeutic efficacy. It is not a far-

fetches guess that: because many of Freud's contemporaries had an inkling of the inefficacy of their own approaches, as well as of the lack of justification for their own claims, they were reluctant to attack *this* variety of claims. And Freud was keenly aware of this attitude of theirs. He rightly felt that there was little risk about asserting such claims.

§531. According to GW-I:449/SE-III:212, hysterical symptoms will as a rule emerge at the age of 8. In §9 we have seen that Freud's real justification for his recurrent claims about the significance of the age of 8, is based on magical numerology. Note further that Freud's postulation is not in the least supported by the biographies listed in §243. Nor is the postulation easy to reconcile with the theory that each hysteric necessarily has symptoms which do *not* derive from childhood.

*Gesammelte Werke* contains a total of one named patient for whom a seduction history is postulated: Aurelia K. She appears in *Studien über Hysterie* under the pseudonym "Katharina": This is in itself a telling fact. In a future volume we shall take a look at the many deliberate untruths in the case-study of this young girl.

I may remark in passing that it is psychologically impossible that Freud would have retracted the seduction theory, if he had had—or in good faith had believed he had—so powerful evidence as he claims to have.

§532. Formally, psychoanalytic theory has been a prominent source of inspiration to many fictional writers. I shall say more about this circumstance in future volumes. Here, I shall discuss one of the few novels on infantile seduction known to me, Tove Ditlevsen's *A child Has Been Harmed* (in Danish).

The academic community has granted a strange privilege to psychoanalysts: they alone are permitted to prove their interpretations and theories by the invocation of secret observations. Try to imagine the following pattern. After having read *A Moon For the Misbegotten* a literary critic may object that Eugene O'Neill has not proved that James Tyrone wanted to sleep with his mother. O'Neill may answer that he is in the possession of an enormous wealth of secret information about James Tyrone. And if the skeptics only knew *this* body of facts, they would realize that O'Neill's conclusions are justified. Try to imagine that, after O'Neill had emitted *this* argument, most people would arrive at the verdict that the objection had been effectively rebuked.

Actually, literary critics will assess whether personalities, social relations and the development of the action, are convincing, on the basis of the manifest data presented on the stage or in print.

§533. Many fictional writers have undergone a personal psychoanalysis, and have testified that they have in the most direct and conclusive way experienced the truth of psychoanalytic theory. They are possessed by the aim of propagating and illustrating this truth. Nonetheless, they have failed miserably. After having read no more than 50 pages of psychoanalytic literature, many amateurs would be able to produce descriptions which, as regards the psychological quality, may match those produced by the greatest writers.

§534. In Ditlevsen's novel, a girl was seduced at early pre-school age by a foreign-adult male. There may be legitimate reasons for not indicating the nature of the act; but apparently sexual intercourse was not involved.—Because of this infantile experience the girl felt neurotic during her entire childhood and early youth. She was unable to feel or show tenderness toward her mother. Nor was she able to have any (platonic or consummated) relations with boys.

We are told that the mother repeatedly and brutally beat the child, not stopping until the latter got convulsions. I do not quite grasp how Ditlevsen managed to overlook the possibility of a causal connection between the mother's cruelty and the girl's personality.

When the girl was about 20, she met the seducer by chance. Improbably as it may seem, both immediately recognized each other. But this pattern should perhaps be considered a legitimate novelistic device for bringing out the plot.

The girl and the seducer talked things over. He informed her about the specific nature of the infantile event. And then she was immediately cured. Immediately, she experienced emotional warmth toward her mother, and caressed her physically.

Ditlevsen's novel illustrates an important and universal feature. While psychoanalytic theory may evoke a strong motivation to write a psychoanalytic novel or dramatic play, this theory will supply no guidance at all for the novelist or dramatist.

No more than common sense is needed for realizing that writers would not have been at such a loss of inventing non-trivial and non-absurd patterns, if any genuine psychological knowledge could be found in *Gesammelte Werke*, or could be perceived by a personal psychoanalysis.

§535. In later volumes I shall discuss Freud's sources, some of which have never been pointed out before. In the present context I shall merely show that the tuberculosis model of the anxiety neurosis is much less original than some commentators imagine.

In his *Compendium der Psychiatrie*, Kraepelin (1882) discusses various *predispositions* of neuroses and other diseases. Half a dozen pages (from p. 61 onwards) are devoted to the factor of *heredity*.

On pp. 46f. the factor of strong emotions is discussed. And on p. 60 the list includes "bodily over-exertion", (bodily) hardship, (psychic) "hardships", (psychic) "excitation", "insomnia".

Freud's originality consists mainly of two things. He has *neglected* the majority of predisposing factors listed by Kraepelin. And: out of factors which may *sometimes* exercise a palpable influence, he has fabricated universal *generalizations*.

It is impossible that Freud was not acquainted with Kraepelin's textbook, of which 5 progressively more comprehensive editions were published between 1887 and 1896. (I have glanced at 4 of these.)

I shall also discuss Charcot as a possible source. Freud visited Charcot from 20 October 1885 to 23 February 1886. But his own claim that he *studied* with Charcot is not true.

Note, for *my* task it is of no consequence whether or not Freud learned the ideas of the tuberculosis model directly from Kraepelin and Charcot. These ideas were widespread within the psychiatric profession.

§536. Let us take a look at Charcot's (1987) *Tuesday Lessons*, originally published in 1887.

The often repeated claim that Freud was the first one to suggest that emotional experiences might produce emotional disease, should be compared with the fact that Charcot clearly acknowledged this etiological factor, but also realized that the significance of the latter had been exaggerated. He says, it is the habit of unsophisticated lay men to explain all kinds of ailment as the result of a frightening event.

As I have said elsewhere, it is Freud's recurrent policy to borrow ideas from traditional lay thinking, promote them into scientific research results allegedly based solely upon clinical observations, and eventually present them as entirely original interventions.

I will quote an important section by Charcot:

"We often speak here of emotions. They are an important etiological element in many neurological disorders. But we must not abuse them. Do not forget that patients themselves often develop their own theories, and these are not necessarily based on proper interpretation of the facts. In cases of epilepsy, I have previously drawn your attention to the effort that families make to demonstrate that such a condition has never before been seen in their family as far as memory can be traced. They, therefore, conclude that the disease definitely relates to a specific incident, perhaps a frightful episode which in most cases never really occurred. Man, as I have said many times, does not like fatality. Each person does his utmost to shun the idea. And so families, like individuals, form a consolidated front of silence. Or else your questions are answered with tales that are based on lies, sometimes not even conscious ones. 'There was a mad dog in the area and the child was frightened.' And the child himself will sometimes repeat the story fully believing it true since he has heard it and has told it so many times.

The doctor, whose duty it is to get to the bottom of this problem and to see things as they truly are, cannot be duped by these storytellers. Investigating the hereditary aspect of neurologic disease is among the most difficult tasks to do well. It demands tact, finesse, and patience. Often it is best to abandon direct confrontation and to use more indirect means of acquiring information." (Charcot, 1987:166) [Q-536:1]

§537. At least three aspects are commented upon here: multiple causation, emotional etiology, and hereditary factors. As regards the second aspect, I shall present two further quotations.

"Tradition dictates that the patient was in the cold with a draft on his face and was perspiring. The seventh nerve trunk was thereby damaged, with all or several of the innervated muscles affected. [...] This is where clinical experience becomes important, as this chill and draft are often a complete fabrication. When a patient is asked if he caught a cold, often he responds, 'That is what they said so I guess it is true.' That is how these tales get propagated. It is the same way with childhood epilepsy. 'It's all because he was frightened.' When you interview the parents about when the supposed frightening episode occurred, it usually preceded the seizure by three or four years. In other words, the child was never really frightened in the first place, and fear had nothing to do with the epilepsy at hand. *It is all folklore.*" (Charcot, 1987:7, italics added) [Q-537:1]

"Trauma can unveil almost any illness to which an individual is already predisposed. Just as scarlet fever can lead to rheumatoid arthritis, trauma can provoke gout." (Charcot, 1987:9) [Q-537:2]

Here we have a close counterpart of Freud's "precipitating causes".

On p. 110 the English editor mentions the fact that Charcot entertained the view that the true cause of *hysteria* is of an emotional nature.

As for hereditary factors, various comments by Charcot or his editor may be found on pp. 77, 80, 95, 147f. and 156. I shall quote only one excerpt.

"You will note how difficult it is to establish hereditary patterns with patients. People never pay much attention to such issues by themselves." (Charcot, 1987:75) [Q-537:3]

In a sense Freud was rather disingenious in postulating that he had obtained family trees for each and all anxiety neurotic patients.



## **Third Book**

### **The Contemporary Aftermath of The Seduction Theory: Incest Trials in the Courts**

## General Issues

*A crank who predominantly deceives ordinary people and makes prophesies on the basis of urine; certainly, such a man deserves much more to be put in a pillory and be thoroughly flogged, and to be sent to a penitentiary, than another who has stolen something because of need and misery.*

Johann Friedrich Struensee

§538. The major part of the discussion of the practical implications of my research must be postponed to later volumes. Inter alia the implications for psychological therapy of neurotic ailments: much human suffering could be remedied almost over night, if psychodynamic treatment were substituted with responsible approaches.

But one aspect need be emphasized already at the present place. In previous chapters I have suggested that the aim of the innovations by Alice Miller and Jeffrey Masson is to provide job facilities at the incest clinics for underemployed psychotherapists.

If interpretations are primarily given in the consultation room, the nature of the belief thrust upon the patient may not be highly significant—e.g., whether he is said to be unconsciously homosexual, or to have been sexually abused in the cradle.

§539. Unfortunately, many psychiatrists and psychologists testify as expert witnesses in judicial trials. They may prove that this or that child or teenager was sexually abused, by means of the very same kind of deductive procedures applied by Freud to establish that G.de B. and Michael had been abused. These procedures are explicitly advocated in two of the most influential books in the field, Mrazek & Kempe (1981) and Sgroi (1982). Because of such testimonies, thousands of flagrantly innocent individuals are sent to prison, frequently for many years.

I do not deny the existence of writings of an impressive quality—for instance Maisch (1972) and Meiselman (1979). But the overwhelming majority of contemporary papers on sexual abuse should never be tolerated within academic discourse.

§540. Within the field of treatment of neurotic disorders, psychodynamic therapists are put on the defensive for decades. To survive, they have no choice but to enter an entirely new field. They have found one. When they occupied a hegemonic position within the field of psychological therapy, they ardently

fought to prevent any efficacious approach from being applied to patients desperately needing them. What they are doing now in the courts, is a continuation of the same willingness to sacrifice human happiness for other aims, which they have manifested since the 1890s.

It might be objected that psychoanalytic interpretations used as judicial evidence, is only marginally related to the main subject of the present report. I wish it were so. But I do not believe in the possibility of cleaning away this theory from psychiatry and psychology, if it is widely applied in courts and at incest clinics. Numerous academicians may reason as follows. It is a fact that thousands of people are indeed convicted on the basis of psychoanalytic interpretations. But it would be highly unethical to send a father to prison for 8 years on the basis of such evidence, if these interpretations were not valid. Consequently, they are valid.

§541. There is little reason to bother much about the difference between incest and sexual abuse. I shall coin the term "*incest ideologists*" about persons who are prone to detect sexual abuse about numerous perfectly normal children, and to fabricate evidence of abuse out of thin air.

§542. A comparison of the situation which has during the 1980s and 1990s befallen the entire Germanesque world, with the witch crazes of the 17th century, may not be appropriate as regards the nature of the punishments meted out. But it is appropriate as regards the nature of the evidence deemed sufficient to justify a conviction. Most Swedish and Danish judges will apply the formula: *to be accused is to be guilty*. Only in exceptional cases could the accusation by a psychologist or an alleged victim be matched by even a perfect alibi, by the physical or logical impossibility that the postulated acts could have taken place, or by the unambiguous indication that the victim or the psychologist had not told the truth.

§543. According to the incest ideologists, scarce attention was formerly paid to the phenomenon of sexual abuse. But as soon as the attention was directed toward this field, an abundance of abuse and misery were discovered. In a moment I shall show that this explanation is contradicted by the empirical facts. But I wish to emphasize another circumstance. The incest ideologists have no choice but to invent *some* explanation as to why the number of incest cases have suddenly been multiplied. And few explanations other than the one they have actually advanced, would have much persuasive power.

§544. The main reason as to why the explanation just mentioned could not be true, is this. The acknowledged international experts in the field are incapable of finding out whether a certain child has been abused. Consequently, it is logically impossible that their conviction of the enormous frequency of abuse, could be based upon any discovery of any empirical frequency. I shall now turn to this important topic.

§545. The psychoanalyst Anny Katan (1973) got a female patient over 30, who had already undergone several unsuccessful psychoanalytic treatments. The patient thought the previous analyses had failed, because she could not stand being alone with a man who was situated behind her, outside her sight. Katan ap-

plied the principle of similarity to this pattern, and deduced that the patient had during preschool age been sexually seduced by an adult male, with whom she was alone, and who approached her from behind, outside her sight.

§546. This woman had seen nothing objectionable in her husband bathing naked together with the 3-year-old daughter. But Katan strongly disapproves of children ever seeing any of the parents less than completely dressed. The risk is however not that the father might become sexually aroused at the sight of the naked daughter—but that the daughter might become sexually aroused at the sight of the naked father. Katan thinks that most 3-year-old children masturbate, and that female children may thereby have fantasies of castrating their father.

§547. This example unambiguously proves that Katan is not competent of disclosing whether a child has been sexually abused. However, many would consider Patricia Mrazek the greatest expert of the field. But if this alleged expert had had the appropriate competence, she would immediately have seen that Katan does not have it. Nonetheless, Mrazek & Mrazek (1981) have completely accepted Katan's deduction. Even more, they have distorted the content of her paper: allegedly, Katan found that some mothers will permit their husband to abuse their daughter, *because* these mothers themselves were abused.

A printed text is somehow permanent and can be re-read many times. The words uttered during a clinical interview are transient, hence easy to miss and unable to retrieve. A person who is unable to perceive correctly the content of a printed paper, must be expected to have much more difficulty in perceiving the features of a patient.—The example just given is by no means exceptional. A very considerable proportion of the patients of the studies listed in Table 1&2 in Mrazek & Mrazek (1981:242f.), were no more victims of sexual abuse than Katan's patient; nor did they have the indicated symptoms; nor are Mrazek & Mrazek's accounts of these studies correct.

§548. Sgroi (1982) is another acknowledged expert whose inferences are likewise derived by means of the principle of similarity. Some young children may, if they are prevented from masturbating in a more direct way, insert a toy into their pants. Thereby they can produce friction by manipulation on the outside of their clothes. If we do not apply the illusion of separation, we may expect a toy with joints to be more efficacious; and if a doll is chosen, more pleasure may be produced if the head rather than the legs is turned against the vagina. But Sgroi applies the principle of similarity and the psychoanalytic standard operation procedure. Thereby, the masturbation pattern is taken to prove that the child had previously been exposed to cunnilingus.

§549. Just like Mrazek & Mrazek would never have accepted Katan's results, if they had been able to decide what kinds of experiences their own patients had had; so, the thousands of clinicians of the field would immediately have realized that the teams around Patricia Mrazek and Suzanne Sgroi are cranks, if they themselves had understood how to identify incest victims.

To sum up so far: it is logically impossible that the present concern with sexual abuse derives from the empirical discovery of any high empirical frequency.

Clearly, the irrational decision to detect such phenomena preceded the false postulation about the actual discovery.

§550. I have already outlined the only hypothesis hitherto formulated, which is compatible with all the facts. Psychoanalysts and psychoanalytically oriented psychotherapists got into economic difficulties, when the existence of behaviour therapy could no longer be concealed. And when it became clear that their campaign against behaviour therapy was about to fail, they started to look for new markets. And only one prospect did not turn out to be abortive.

But two alternative hypotheses need some comments, viz. that the decisive etiological factor was the propagation of the feminists, or of the religious fundamentalists. No doubt, the fundamentalists have joined the trend with enthusiasm. But their influence is in most countries much too small for initiating such a trend.

§551. It could be argued that the incest ideology would never have become as predominant and long-lasting without the zealous fight of the feminists. But they could hardly be the initiators. It is difficult to understand why feminists should initiate such a trend exactly at the moment when the latter was profitable for the psychotherapists. Miller and Masson published their books 6 to 9 years after the international woman's year (1975). But the idea that Freud's early patients had indeed been sexually abused, was well under way several years earlier. Katan (1973) is just one example among others.

It is even more difficult to understand why the feminists should bother to dig out Freud's papers of 1896. And in particular: *why* should they completely *distort* the content of these papers?

§552. Before presenting concrete cases, international readers may need some information on the special position of witness psychology in Sweden. *The Laboratory of Witness Psychology* at the University of Stockholm was created by Arne Trankell. His successor, Astrid Holgerson, has refined the methodology of estimating the trustworthiness of statements by witnesses, defendants and suspected victims. Not least, she has developed the pedagogical aspects of the technique.

Since some time the incest ideologists have gained complete control over *The National Board of Health and Welfare*. And the latter is zealously trying to remove any obstacle to the conviction of any male accused of sexual abuse. Since witness psychologists had sometimes shown that a certain account is not trustworthy, while child psychologists had automatically taken the accusation to be true, *The National Board* originally fought for the monopoly of clinicians in handling incest cases.

Strictly speaking, *The Judicial Council of the National Board of Health and Welfare* is a sub-department of *The National Board*. In practice, *The Judicial Council* has repeatedly acted in an independent and objective way. Inter alia, *The Judicial Council* decided that both psychiatrists' judgement of the trustworthiness of persons, and witness psychologists' judgement of the trustworthiness of statements, may contribute to the illumination of a case.

§553. Because of this decision, *The National Board* changed its policy. Hyper-brief courses in *pseudo-witness-psychology* were started, in which psy-

chodynamic therapists were taught how to imitate the mere *jargon* of witness psychology, so as to deceive judges. In Sweden it is often claimed that there are two "schools" of witness psychology; or that a course of witness psychology is held at *The University* of Lund. Actually, it is a matter of a commercial course for which the Association of Psychologists is responsible. Since the fee is usually paid by the hospital at which the student is employed, there is a scarce chance that anyone who is not an incest ideologist will be accepted as a student. Without exception (cf. however §696), these pseudo-witness-psychologists function as commissioned assistants to the prosecutor, and will invariably arrive at the conclusion that the suspect is guilty. (The exceptions are too few and too odd to merit any comment.)

§554. *The National Board* has specifically recommended these people. And a list of them is circulating in all courts. At the same time the Board has for some years ardently attacked the genuine witness psychologists trained in Stockholm. Judges are asked not to pay any attention to what they may say.

It is by no means my position that the Stockholmian group has made no serious mistakes. I shall document such errors. But none of them has a record even remotely comparable to that of every member of the Lund group.

§555. And now I shall turn to the description of concrete cases. Not every case is heavily loaded with psychoanalytic interpretations. And even if a case is so, I shall feel free to focus upon other sections of the evidence. Nonetheless, the overwhelming majority of those experts, whatever their specific profession, who deemed any of the suspects guilty, are proponents of psychodynamic views.



## The Cases of Carola, Odenmark, and Synnöve

*An actor from the competing troupe at l Hotel de Bourgogne, Montfleury, handed to Louis XIV a petition in which he accused Moliere of having married his own daughter.*

Georg Brandes

§556. Most names and dates of the cases to be described, are fictive.

— Four-year-old Carola asked her father whether she could suck his penis. He answered that such things are nasty. A pseudo-witness-psychologist asserted that Carola could never have learned about the existence of fellatio, except by having been exposed to this variant by the father. A genuine witness psychologist soon disclosed that the child had repeatedly been present while her mother had performed fellatio upon her lover. The father was acquitted after having been arrested for one month. In the eyes of the social welfare agency he is nonetheless guilty. And to this date, after 7 years, he has been prevented from seeing his daughter.—It is no accident that the same child psychiatric clinic is involved in the cases of Carola and Embla.

§557. After the divorce, the social welfare agency deemed Mr. Odenmark the more appropriate parent, and recommended the district court to give to him the custody of the three children aged 3 to 7. But then the mother reported him for sexual abuse. A child psychologist, Nordvall, wrote a pronouncement in which she asserted that incestuous assaults had unambiguously occurred. *On the basis of Nordvall's report* a police investigation was started. A genuine witness psychologist (Astrid Holgerson, the leader of the Stockholmian Laboratory) was appointed. Inter alia, she asked Nordvall about the observations and grounds upon which her conclusion were based. Nordvall's answers are tape-recorded, and I shall cite all her grounds. 1. As for observations, she did not list any at all, but merely supplied an abstract heading. Her primary justification for the verdict that the father was guilty, consisted of "those things which *the mother* had told" about what the children "had" said. Nordvall had talked very briefly with the children. And her purpose was merely to "*confirm the mother's accusations*". 2. Nordvall had made a telephone call to a colleague who is "a specialist" of sexual abuse. This colleague had said that sexual abuse may cause serious harm. And this abstract formula constituted a second proof that Mr. Odenmark had abused his children. 3. Nordvall had told the expert that Odenmark seemed really to love his

children. The expert had answered that it is just such fathers who abuse them. 4. Nordvall had obtained much support from her fellow workers in her attempts to prevent the contact between the father and the children. 5. Nordvall had at an earlier time ( $t_1$ ) produced a pronouncement in which she had declared the father guilty. *On the basis of nothing whatever but her verdict*, the police had subsequently at  $t_2$  started an investigation of the case. Nordvall explicitly stated that *one of her grounds for arriving at her verdict at  $t_1$  was the fact that the police had at  $t_2$  started an investigation. And a police investigation is not undertaken lightly.*

§558. Actually, the children denied having experienced any indecent behaviour. Thanks to Holgerson's investigation Odenmark was never tried.

But the whole process had taken years. And the social welfare agency decided that, since the children had been living with the mother for such a long time, they should remain there.

For years the mother used Nordvall's pronouncement to prevent the father from seeing his children.

§559. Third case. After the divorce the mother had the custody of 5-year-old Synnöve, and the father of 7-year-old Carl. The girl received psychotherapy at the clinic. After three months the therapist had indoctrinated her sufficiently to venture a police report. A video was made, *on the basis of which the prosecutor arrested the father*. According to Swedish law an arrest must be confirmed by the court within a few days. The prosecutor tried to have the arrest confirmed by means of a brief summary of the content of the video. But the defense council zealously fought for having the unabridged video shown; and he succeeded.

§560. This is the content of the video. The 5-year-old child is exposed to a fire of injunctions by as many as three persons: the psychologist, the mother, and a police officer—"Tell us what you said previously about what Daddy has done to you." Synnöve remains silent.

Finally she is given a set of anatomically correct dolls. And then she starts. I fucked my brother, and Daddy fucked Granny. The psychologist tears away the Granny-doll and says that Granny was not there. But Synnöve tears it back and affirms that all four were fucking together.

The cause was removed from the cause list. However, the prosecutor and the psychologist are still working with the task of having a man, whom they *know* to be innocent, sent to jail on the basis of forged evidence.

## The Cases of Embla, Betsy, and Violet

*What Max Scharnberg, who has not met Embla, has contributed to the elucidation of the case, is by the Court of Appeal deemed to be of a very limited value.*

Sven Larsson & Johan Stenberg &  
Karin Jonsson & Arne Danielsson  
& Lennart Pettersson

*When evaluating Abendstern's report the fact should be specifically taken into account that she has a long experience of making investigations as regards children's reactions in various situations.*

Sven Larsson & Johan Stenberg &  
Karin Jonsson & Arne Danielsson  
& Lennart Pettersson

§561. According to the official standpoint, it is not the obligation of the defendant to prove his innocence. It is the obligation of the prosecutor to prove his guilt. In actual practice, it would be hard to find a judge in Sweden (and possibly the same thing is true in many other countries) who does not apply the very opposite principle. Even more, most Swedish judges would not hesitate to convict a defendant despite foolproof evidence of his innocence.

In the entire history of Sweden, no abuse case has been so extensively and intensively illuminated as that of 14-year-old Embla. Not one single circumstance can be found, which may provide the slightest indication of incestuous assaults. At the same time, more than half a dozen non-overlapping sets of circumstances exist, *each of which alone* constitutes a conclusive proof of the innocence of the father. Nonetheless, no less than 15 judges of three courts have unanimously convicted him and sentenced him to 2 years 4 months in jail.

§562. On the Sunday before Christmas Embla visited her schoolmate Jane. The girls saw a section of the TV series *Degrassy Highschool*. The intended age level of the series is that of the first kisses. A girl named Liz tells a female comrade that she was abused at the age of 11. Her comrade consoles and caresses her.—Now, Embla is more fond of company than most teenagers. But she did not have a close friend. Apparently, she felt it would be wonderful to be consoled and caressed by Jane. She promised Jane a letter on the following day.

§563. Certain sentences in this letter are exact plagiarizations of the TV program. The letter primarily says that Embla's father had slept with her. I surmise that she gave no thought to the possibility that anyone might be harmed. But after

a few hours her father was arrested. And now Embla was confronted with the choice of losing her face in the eyes of her schoolmates, or of sticking to the story. In general, she chose the latter option.

When she finally admitted that the allegation was "bluff" (her own word), she was submitted to prolonged interrogation, first by the social welfare agency, and then by the police. The former interrogation is classified, but the latter was tape-recorded. The police officer strongly reproached her for being a bad girl if what she previously said was not true—whereafter Embla switched back to her first version.

The assaults as such were not thought immediately to necessitate psychotherapy. But the retraction led to her being given a psychotherapist on the very day of the retraction.

Her mother had taken a strictly neutral attitude. (Later the pseudo-witness-psychologist Abendstern strongly reproached her for not having been dogmatically convinced of the truth of the accusation, in advance of the emergence of any evidence.) But because of the mother's neutral attitude, the social welfare agency moved the girl to another family immediately after the retraction.

§564. Eventually, Embla elaborated her narrative. She had had sexual intercourse with her father some 40–50 times; and all acts but one had been performed *in the missionary position*. But then the gynecological examination revealed that Embla was a virgin. The judges avoided being clear about what they think happened. But I take it that they thought the father might have touched the exterior part of the daughter's sex organ with his penis (but would the missionary position be very appropriate for such an activity?); and that Embla, since she did not know what coitus really is, might have thought that this is coitus. Anyway, it is a highly frequent occurrence that judges invent numerous ad hoc auxiliary hypotheses in order to accommodate the facts to their prejudiced view.

§565. Because of a traffic accident, the father's elbow was broken and infected. For more than a year his left arm was put in plaster. It is a sheer impossibility that he could ever perform sexual intercourse in the missionary position.

The judges of the Court of Appeal seemed to think that any testimony by a voluntary sexual partner should not be given much credence. And intensive bodily pain might not have stimulated him to use a different position.

§566. A reconstruction of the crime has been video-taped, inter alia of the assaults in the car. Embla's lawyer acted the role of the father, in accordance with the girl's momentous instructions.—The video unambiguously reveals Embla's pervasive ignorance as to how sexual intercourse is performed. The father must have had his penis on his left knee or so, in order to reach the daughter's sex organ.

A clinical psychologist and his wife made another video. They literally tried to follow the instructions, that is, they actually took off their trousers. This video even more illustrates the impossibility of the alleged pattern.

§567. If Embla told the truth, she was trembling for hours after an assault. She also suffered from insomnia, and had nightmares almost every night.—

Nonetheless, her diary during 5 consecutive months, 4 of which preceded the police report, is permeated with a happy and optimistic mood.

Three assaults are exactly dated, and there is no room for the idea that the girl might have mistaken the dates. For one of these assaults the father had an alibi. Another assault was allegedly performed late in the afternoon. But before going to bed Embla started the description of all the pleasures of the day with the following sentence: "Hey and ho! Rubber toe! Today I have had really fun actually."

§568. Her *personality* is worth noticing, because it may throw some light upon the problem *how* the false accusation could have arisen. She is highly *extra-verted*, and two features are fundamental in extraverts: the reduced ability to form conditioned reflexes, and the increased need of innovative external stimulation. The latter feature could hardly have been more conspicuous than it is throughout the diary. But the former is more relevant. Still at the age of 13 she had not gained control over her bladder; she wet the bed. And her moral development is lagging behind her age. She has engaged in rather many petty and less petty thefts and is a problem child. Already at the age of 10 she stole the cash savings of the school class. And a few weeks after the trial in the district court she stole clothes in two shops, worth 900 Swedish crowns. When her foster mother wanted to know how Embla had obtained these clothes, Embla said that Granny had given her 500 crowns for retracting the incest allegation.

In her written investigation to the court, the pseudo-witness-psychologist strongly reproached the mother for having tried to do anything about the daughter's inclination to steal.

§569. Because the girl has not yet learned to feel guilty when she is lying, she may fabulate with the most honest facial expression and the most sincere tone of voice. Consequently, she has an increased capacity for convincing other people. As a result, exactly this behaviour of hers is likely to be *rewarded*. And her reduced capacity for motivational learning is more than compensated for by the abundance of learning situations she is likely to encounter.

At present, Embla is definitely not capable of producing a logically coherent fabrication. Like so many other fabulators, she has predominantly focused upon the expressive features.

Unfortunately, she is undergoing psychotherapy, during which she is expected and forced to simulate that she is an incest victim. This treatment cannot easily fail to provide some instruction in the art of constructing detailed fabrications which are hard to disprove.

§570. Embla's personality does not prove that she has not been abused. Extra-version does not function as a kind of prophylaxis against sexual assaults. Nonetheless, the insight into the personality type may yield a deeper understanding as to *how* the false allegation emerged. And first and foremost, this insight could have guarded the judges against the hasty conclusion that a person with such facial expressions etc. could only have told the truth.

§571. It is easily seen that Embla applies a limited number of recurrent techniques for constructing or defending her fabrications. One of these is to adopt the

ideas suggested by others. At first she claimed that every assault had taken place in her own room. But when the police interrogator ignored this assertion and inquired about assaults committed in the car, Embla described two recent such events. Many comparable instances may be found.

She has a limited capacity for inventing non-contradictory details. Therefore, she tries to supply few details, and gives a wealth of don't-know-answers. She also tries to mitigate contradictions by means of "intermediary answers". For instance, while describing the second coitus in the car, she unambiguously claimed that her father had *merely unbuttoned* her trousers during the act. The police interrogator objected that it is impossible to perform intercourse in this situation, and that the trousers must also have been *taken down*. Embla then tried to modify her account so that it would not be impossible; but she also tried to escape contradictions between the first and the second version. She said that her father had not taken her trousers *altogether* down.

§572. In the Court of Appeal she was interrogated by the council defense council for 5 1/2 hours. She could give a satisfactory answer to a total of one question. She repeatedly told the council to direct his questions to her father rather to her. E.g., the father had had intercourse with her without taken down her trousers—hence, *he* and *not she* must know how he managed to do this.

§573. In §§442ff. I described the tendency of many fabulators of producing progressively more comprehensive or extreme fabrications. The series of versions produced by Embla at different times agree with this pattern. Admittedly, progressively more extreme versions may also emerge when a genuine incest victim gradually conquers her shyness of exposing the whole truth. But the latter rule cannot be applied to the *specific* versions at hand.

Two weeks after Jane had received the letter, Embla told the pseudo-witness-psychologist that summer 1990 was very nice, because no assault had occurred during this period. But shortly before the trial in the Court of Appeal, Embla recalled such an assault which was performed in the two-room summer cottage of her adult male cousin. Her father had been lying on the floor, and had reached up to the bed with his hand, and had sexually touched the daughter under the blanket.

§574. Two entire families comprising at least 8 persons were present. The father had fallen asleep because of liquor. Numerous attempts by the cousin of waking him up had been fruitless. Possibly as a practical joke the cousin took two photographs. The father is seen lying on the floor on his *right* side, and with his *left* arm put in plaster. His *back* is turned toward the bed in which Embla was lying.

I have tried to perform such a sexual assault upon my niece, lying on the floor on my right side with my back turned toward the bed. I was unable to reach her even with my finger tips, whether I tried to move my right hand backwards or over my left shoulder.

It is easy to advance verbal claims. It is much more difficult to imagine what concomittant features would necessarily have been constituents of the pattern,



if the postulated phenomena had really pertained. The deficient feeling of reality is a highly frequent characteristic of fabulators.

§575. In the Court of Appeal Embla obstinately denied the most obvious facts. A long series of questions were needed to penetrate this stone wall. First she denied that the photographs were taken in this cottage (but eventually admitted it). Then she denied that her father's arm was put in plaster on that occasion. Although she admitted to have been in the cottage only once during summer 1990, she iterated that the photos did not disprove her account, because *she* had not indicated any *specific date*.

§576. The pseudo-witness-psychologist will be named "Abendstern". Her written investigation and her oral testimonies in two courts throw much light upon her way of thinking. Flagrantly, she took the father's guilt as an aprioristic axiom, and picked up a few trivial (real or faked) facts to *decorate* her axiom.

The jargon of genuine witness psychology requires that alternative hypotheses be formulated. And so Abendstern formulated 4 hypotheses, viz. that the father was guilty, and that he was innocent on account of three different patterns. She pretended to refute each of the three non-guilty hypotheses, whereafter only the guilty-hypothesis remains.

Even if she had truly refuted the three hypotheses, the validity of the logical scheme depends upon whether the set of hypotheses is exhaustive. It is a matter of routine to formulate four false hypotheses on the colour of the Swedish flag; to refute three of them; and then to conclude that the fourth is probably true.

§577. Abendstern has also applied a second logical scheme. She thinks that hypothesis H:1 is true, but wants to construct an apparent refutation. She combines H:1 with H:2, thereby obtaining (H:1&H:2). Then she refutes (or pretends to have refuted) H:2. If H:2 is false, then (H:1&H:2) is false. Having thus refuted (H:1&H:2), she pretends to have refuted H:1.

It seems that she felt that the incest allegation was inspired by the TV program *Degrassy Highschool*, and that the father was innocent. Aiming at refuting this hypothesis, she constructed the *compound* hypothesis that the allegation derived from the program, *because* Embla had somehow perceived a hidden conflict between her parents. Thereafter, Abendstern dogmatically claimed that there is no such hidden conflict. And she pretended that she had by this procedure established that the allegation was not inspired by the program.

§578. But whatever circumstance she noticed, the latter was said to indicate that the father was guilty. At first, the father could have been innocent *if* there had been a hidden conflict. In the Court of Appeal she made a volte-face. Here, she claimed that there is indeed such a conflict. But exactly this circumstance points toward the father's being guilty—because sexual abuse is particularly frequent in families with a hidden conflict.

§579. Everything she said or wrote was *rhetoric*. She proved the truth of the incest allegation from the "fact" that Embla had previously written other letters, in which she wanted to expose the abuse; but she had destroyed these letters. During cross examination Abendstern admitted that she had only Embla's word

about the former letters. It did not occur to her that if the assaults were fabricated, then the letters must likewise be so.

§580. Another proof was the "fact" that Embla suffered from irregular menstruation because of the abuse. But when it turned out that monthly dates indicated in the diary were perfectly regular, Abendstern answered that this does not matter. The important fact is that Embla was afraid of being pregnant. How did she know that the girl had told the truth about this fear?

§581. The thefts were interpreted as a consequence of the abuse—hence also a kind of proof of the assaults. Like Freud, Abendstern did not hesitate to reverse the temporal relations. The very first assault allegedly occurred when Embla was 11 years old. But her thefts reached a first peak when she was 10.

§582. One more illustration of Abendstern's *rhetoric*. In the Court of Appeal she claimed that the main difference between her and another expert witness was that he reasons like a laboratory psychologist, while she is a clinician. The other expert solely pays attention to *the statements*, while she pays attention to *both the statements and the person*. And if a statement is seen in the light of the person, it is more easy to formulate fruitful hypotheses.

But in the district court Abendstern made it clear that it is a fundamental feature of her methodology always to formulate each and all her hypotheses *before* she has met any of the persons. (I shall disregard the fact that this is a preposterous methodology.) Moreover, it is easily seen that none of her hypotheses takes any account of the personality of Embla, nor of any of the other persons. At the same time, the other expert witness paid excessive attention to Embla's personality, and to the interaction between her personality, her allegation, and her persistency in fabricating.

The Court of Appeal was much impressed by Abendstern's contributions.

§583. I have again and again emphasized the fact that as much harm may be done to the false victims as to the alleged offenders, if the accusations are treated as if they were true. Markedly different negative consequences may arise. Embla's primary problem is her reduced capacity for forming conditioned reflexes, and her retarded moral development. Lying, stealing, insensitivity to the suffering of others are predominant. And now those properties which she most of all need to conquer, are reinforced and promoted. She has got damages of 100 000 Swedish crowns. Her popularity among her schoolmates has become excessive. Her schoolteachers now treat her with considerable sympathy. A dozen psychiatrists, other doctors, psychologists, social workers, policemen and the prosecutor, and no less than 15 judges of three courts, have encouraged a variety of behaviours which, if she had been only one year older, would have been termed criminal. Previously, both the school, the social welfare agency and the police were concerned about her thefts. But now any such transgression is immediately excused.

Embla has got entirely wrong signals as to what kinds of behaviours are approved by society. The worst might not happen. But if Embla should eventually have a truly criminal career, the judges and psychiatrists etc. must accept their share of the responsibility for the outcome.

§584. Next to the case of 15-year-old Betsy. She was highly depressive. A friendly and neutral remark by a schoolteacher might make her weep. The idea occurred to the school nurse that the depression might be the result of incestuous assaults. The school nurse, the school welfare officer and a social welfare secretary interrogated Betsy for months. Finally, she submitted to the pressure and said that her father had raped her 6–8 times during the last two years. Her gradual submission is documented in case-notes. The district court sentenced the father to 8 years in prison, and damages of 45 000 Swedish crowns. The Court of Appeal reduced the prison sentence to 4 years.

§585. Betsy described the last assault as follows. She was raped in the evening. On the very next day she went to school. After homecoming from school on that day she felt so desperate that she tried to take her life.

This pattern is so coherent that there is no room for the hypothesis that Betsy might have mistaken the date. But the reference to the suicidal attempt enables the almost exact dating of the assault. On 9 September 1988 Betsy visited the social welfare agency together with the school nurse. This was the very first occasion at which she learned about the suspicion of sexual abuse. At that date she had neither a scar nor a bandage around her wrist. On 12 September the school nurse shifted the bandage for the first time.

§586. The crucial fact is that 9 September was a Friday. Consequently: either Betsy was raped on Friday night, *went to school on Saturday*, and cut herself with a razor on Saturday afternoon. Or else, Betsy was raped on Saturday night, *went to school on Sunday*, and attempted suicide on Sunday afternoon after homecoming from school.

§587. Moreover, Betsy had on 8 September moved to a foster family. This had happened with her father's consent, and because of an unrelated and neutral reason. The foster family has mapped out everything Betsy did on Friday and Saturday. There is simply no room for her having met her father.

§588. It is not a far-fetched guess that Betsy's intention to take her life was a response to the attempt by the social welfare agency of forcing a false accusation upon her.

§589. The girl eventually handed over a series of letters she claimed to have written after the assaults. She has *always* stuck to the claim that the assaults started *after* her mother left the family (on 28 February 1986). But the first letter is dated "6.4.1984". Even orally she has repeatedly stated 1984 to be the year. We have three indications of the time of the first assault: in 1984; at the first weekend after the mother had moved away; and some four months after the mother's departure. Strangely, a letter describing the *third* assault is dated "May '86".

§590. Actually, Betsy experienced a deep trauma in 1984, when the family moved to another town. She was uprooted from her circle of friends, and came to a school where such circles were already established, whence she came to be an outsider. However, a different version is supplied by her schoolmates, who think they have indeed approached her, while she has rebuked them.

§591. In the beginning the letters were presented as the original documents.

But it soon turned out that the handwriting of the letters did not agree with the handwriting in Betsy's school books from 1984 and 1986; but much more with the handwriting she used in 1988. The girl then changed her version: the letters handed over were copies she had recently produced, and she had lost the original letters when she moved to the foster family. Presumably her father had found them and destroyed them. In the court she also told the prosecutor that her father had stolen and destroyed her diary. Thereby, both Betsy and the prosecutor were perfectly aware of the fact that Betsy had handed over the diary to him. (Not until in 1992 did I learn that the prosecutor had concealed this crucial piece of evidence.) Her foster mother has confirmed that Betsy's diary followed the calendar year, and that she arrived in September with the diary of 1988.

§592. In the letter dated 6.4.1984 it is stated, twice and very explicitly, that the described assault was the very first one. But the discrepancy is enormous with her oral description at the police interrogation of the first assault. Betsy then said that the letter describes the second assault: But this remedial device merely shifted the place and not the size of the discrepancy. A third version was that the letter is a paraphrase in which different details are taken from different assaults.

§593. Certain sentences in the letters are plagiarations taken from a TV program (*Studio S: En skam utan like*, "Studio S: An Unparalleled Ignominy"). Betsy was very young when this program was originally shown. Hence, the only reasonable explanation is that a video of the program was available to the social welfare agency, who showed it to the girl during autumn 1988 in order to break her resistance. An inescapable consequence is that both the social welfare secretary and the school nurse recognized the plagiarized sentences. Hence, they were aware that the letters were faked.

§594. An assistant professor of psychiatry and chief physician—who will be named Dr. Leonard—testified that he had performed a sufficiently comprehensive psychiatric investigation of the trustworthiness of the girl. He assured two courts that she had almost certainly told the truth. He even presented a concrete proof. He had asked Betsy whether she had made a false allegation because she was jealous of her father's new girl-friend. Betsy had answered "No". And this brief answer of hers proves that her accounts about the abuse are true. Dr. Leonard explicitly vouched for the truth of the claims that the abuse had started *both* in 1984 *and* after the mother's departure.

§595. The Court of Appeal accorded strong evidential power to the "fact" that Betsy had told Dr. Leonard about the assaults, despite his total ignorance of the existence of the suspicion of the girl being an incest victim. Dr. Leonard committed perjury. The social welfare agency had explicitly informed him in a letter. And the social welfare secretary was present at his first meeting with Betsy.

§596. When this psychiatrist later had to defend himself to the Parliamentary Commissioner for the Judiciary and Civil Administration (the Ombudsman), he made a volte-face. Now he claimed never to have made any investigation at all. He had from the very beginning considered the data much too meagre to justify any conclusion. In the courts he had solely stated his subjective view as a lay-

man. And he took for granted that the judges had understood his testimony in this way. He claimed to be innocent because of an additional reason: he had *in private* informed *the prosecutor* about all those things he concealed from the judges.

§597. There was an unusually tender and enviable relation between this father and daughter, which for years survived the conviction. Betsy has written a lot of letters to her father in the jail containing sentences such as the following: "I love you most in the whole world. You are more than a friend, much more." These letters are formulated as if the father were enjoying himself at a holiday place. The girl seems to have little understanding of what she has done.

§598. Two new trial motions have been handed to the Supreme Court. The presentation given here is a very brief summary of the content of these motions. *The Supreme Court (Jermsten, Gregow, L.K. Beckman, Sterzel, Munck)* has ruled that exactly this pattern of circumstances is such that it should lead to a conviction. However, one judge referee of the Supreme Court suggested that the case should be re-opened, since the proofs of the father's guilt are conspicuously inadequate.

§599. I have been struck by the considerable variability of the nature of the cases. Violet's stepfather Georg had left his wife Rosa for a younger woman, and Rosa decided to revenge herself. She pressed Violet, who was now 17 years old, to report Georg for having abused her since she was about 12 years old.

Violet obediently went to the police, but she did not perform the task very well. She could supply no details at all about any aspect. I shall quote two sections from the first police interrogation.

"The interrogator points out to Violet that she has been very vague as to the details or rather the occasions. Could she herself supply any explanation as to why this is so?

Violet: 'If only I knew.'" [Q-599:1]

[Interrogator:] " 'But dear little Violet, isn't there any event you could connect things with so as to arrive at any specific occasion? What I am thinking of is, if it was your birthday, if something special had happened in the family, or if a friend of yours had made a call, or something of the kind. If you could search your memory for any such things to connect with some of the assaults, in time and also in execution.'

Violet shakes her head and says that she cannot do this." [Q-599:2]

We shall later see what an extraordinary memory Violet demonstrated in the courts.

§600. The police were highly skeptical of the allegation. Violet was sent to another assistant professor of psychiatry and chief physician. She will be named Dr. Paulus. The latter immediately announced that the girl had told the truth. During her testimony in the Court of Appeal she presented a list of 6 proofs, each of which was claimed to establish the reality of the assaults. I shall list all of them.

*Proof A.* Violet had RECOUNTED that she had never previously dared to tell anyone about the sexual abuse.

*Proof B.* Violet had RECOUNTED that she could not lock any door behind



herself; even when she was taking a bath, the locked door could be opened from the outside with a special key; and Georg had repeatedly intruded.

*Proof C.* Violet had RECOUNTED that she had experienced gifts, such as a bicycle she got when she was about 12 years old, as bribes to make her accept the abuse.

*Proof D.* Violet had RECOUNTED that she had felt threatened, hunted, and frightened.

*Proof E.* Violet had RECOUNTED that she had nightmares.

*Proof F.* [As for this last argument, Dr. Paulus's words should be literally quoted.] "She has told a few details, for instance that she had recognized a pornographic video cassette and her mother had wondered a little how she could know that it was a pornographic video before she had seen it. But the reason was that she had recognized the brand of this cassette. Apparently, she did not need to answer her mother, so the latter did not take any—did not become suspicious at that occasion. But it is this kind of petty—one might say somewhat practical details which make the account seem authentic." [Q-600:1]

§601. It would be a tough job to find a non-academic layman who was capable of producing this variety of arguments, or who would consider them valid. Neither the psychiatrist nor the judges realized that Dr. Paulus has invented a formula which any future fabulator may apply, and the presence of which will automatically transmute any fabricated incest allegation into an account of authentic historical events. It is henceforth a matter of routine for me to prove that I, MS, is an incest victim.

After having had Violet in psychotherapy for a whole year, Dr. Paulus was altogether ignorant of the fact that literally any kind of pre-marital sex was forbidden to the girl, who was not even permitted to be alone with a boy.

§602. In an affidavit the psychiatrist listed 12 symptoms, which allegedly were typical effects of sexual abuse; hence an additional variety of proof. Dr. Paulus has after the trial explained that she had compared Violet's pattern of symptoms with Table 1&2 in Mrazek & Mrazek (1981:242f.), whereby she had found a noticeable correspondence. However, the objective facts are, first, that many of the patients of the M&M Tables were not at all victims of sexual abuse, and did not have the postulated symptoms (cf. §§545–547). Second, that many of Violet's symptoms, and possibly all of them, were simulated. It is a sheer impossibility that Q-599:1 and Q-599:2 were associated with a girl who, on her way to and fro the police station, incessantly turned around because she imagined that her stepfather was behind her. Likewise, Violet's nightmares are manifestly fabricated. Third, the discrepancy between Violet's alleged pattern of symptoms and the M&M Tables is almost maximal. Ninety-four per cent of the table symptoms are missing in Violet, while 42–75% of Violet's symptoms (depending upon the preferred categorization) are missing in the tables.

§603. Moreover, Violet's alleged symptoms (insomnia, nightmares, anxiety, headache, fatigue, dizziness, and so on) are so trivial, that they may be observed



in a wealth of different syndromes. Curiously, the African sleeping-sickness belongs to these.

Although most of these symptoms have not been derived by means of the psychoanalytic *principle of similarity*, it is a Freudian theory that certain symptoms are uniquely associated with certain etiological events, so that the nature of the events can be deduced from the nature of the symptoms.

§604. Because of Violet's poor performance at the police interrogation, Rosa wrote a detailed account of what Violet was supposed to have experienced. And Violet learned the text by heart. But the mother and the daughter made a mistake which unambiguously reveals the origin of the plot. *Violet had learned by heart the sheer verbal formulations.*

A genuine incest victim might prepare herself for the interrogation in the court. She might write a list of entries, so as to facilitate recall and rehearsal of the entire set of events. But at different rehearsal occasions she would clothe the events into different words. The idea would never have occurred to her to stamp in the very word sequences of the written text, in the same way in which an actor would stamp in a monologue to be performed on the stage.

§605. In both the district court and the Court of Appeal Violet started the interrogation with a monologue; 2481 words in the former court and about the same number in the latter. Her two monologues were not literally identical. But almost all sentences followed each other in the same order. Each and all non-trivial discrepancies were closely associated with pauses, which in several respects had the character of an actor on the stage having forgot the next line.

And whenever Violet was asked a question the answer to which she had not prepared in advance, she repeatedly refused to answer and told the defense council to ask her mother instead.

§606. In Table 606:1 I have juxtaposed a brief section from the two initial monologues, together with a monologue from the above mentioned TV program *Studio S: An Unparalleled Ignominy*. (None of the TV monologues are authentic. They have been constructed by psychologists or journalists, and are performed by child actors.) Like Betsy, Violet or Rosa could hardly have recalled a text shown in TV quite a few years earlier. But the family belonged to a fundamentalist church which, years before the general Swedish population, had a strong interest in incest. (It goes without saying that it would have been a poor strategy for Rosa to plagiarize the TV monologue completely.)

§607. There may be a specific reason why Rosa hit on the idea of accusing her ex-husband of incest. A few years earlier one of Violet's schoolmates, a 14-year-old girl, had murdered both her parents. A detective inspector who is particular prone to perceive sexual abuse where it does not exist, invented the explanation that the murderess was an incest victim, who had revenged herself upon the offender and the parent who had knowingly tolerated the abuse. The false explanation was propagated over the entire nation. At Violet's school an enormous campaign was conducted on sexual abuse; and the school welfare officer had a private talk with each and all students.

Table 606:1.

<i>Violet's testimony in the district court</i>	<i>Violet's testimony in the Court of Appeal</i>	<i>The diary of Lena, 16 years old, in the TV program</i>
I know that <b>in the evenings when Mummy was working I sort of was lying up there</b>	And (3 seconds) in (4 seconds)	<b>We were again alone at home, and I had gone upstairs and gone to bed.</b>
and hoping that he would not come up		
and <b>he always used slippers</b>	<b>he always used slippers, Georg</b>	
<b>so that one heard when he was coming up the stairs</b>	<b>so that one always heard when he went up the stairs</b>	<b>When I heard his steps in the stairway, I understood at once that he would come to me. I began to freeze and sweat.</b>
<b>so that one hoped</b>	<b>so that one was always lying in the bed hoping that he —</b>	<b>I always hope</b>
<b>he was just going to see TV or do</b>	<b>that he was just going to see TV or do something of the kind</b>	<b>he will just fetch something and leave again.</b>
<b>or that he intended to tell something to Karen or Lucas about something they had forgotten or so.</b>	<b>or that he would like to tell something to the others that he had forgotten.</b>	
<b>When one heard then that he came up and he closed Karen's door</b>	<b>But when one heard then that he closed Karen's door</b>	
—it was always open because she was very scared of darkness	— — —	
<b>he closed the door</b> [ <i>repetition!</i> ] <b>then I knew that he was about to go to my room,</b>	because he always did that when <b>he was about to go to my room—then he closed the door to my sister,</b> [ <i>repetition!</i> ]	[ <i>cf. the above sentence:</i> ] I understood at once that <b>he would come to me.</b>

§608. The case of Violet illustrates a recurrent pattern. The sheer number of proofs may be crucial in trials of this variety. But the quality of the proofs is of little significance.

Georg was sent to prison for three years, and had to pay damages of 80 000 Swedish crowns. The judgement of the two lower courts were unanimous. The Supreme Court refused to consider the case. Including the first appeal there has been five attempts at re-opening the case. The account given here is a brief summary of the fifth new trial motion, which exceeds 150 000 words. All judges and five judge referees of the Supreme Court have been involved in this case. Two judges and two judge referees have found the evidence insufficient for a conviction. All the others have arrived at the opposite verdict.

## The Cases of Ursula, Rachel, Henriette, Hildegard, and Ingalisa.

*The hangman's fingers are as white as are his slain.*

Chaim Grade

§609. The case of Ursula is more depressing than any of the others, not only because her father was given a more severe prison sentence (5 1/2 years), but primarily because he was convicted on the basis of *secret evidence*. Hence, he was not even given the formal opportunity of defending himself. Some people might find the word corruption appropriate.

In this case there are three categories of evidence. One category was available to all the involved; one was available to the defense council, but the latter was forbidden to inform the defendant about these facts or pseudo-facts and, consequently, was prevented from learning what errors they might contain. A third category was not even available to the defense council.

§610. As for the third category, the crucial fact is not that clinical case-notes were known only to the treating psychiatrists. But these case-notes were handed out to a witness psychologist and a pseudo-witness-psychologist. There is no reason to waste space upon the deductions and results of the latter. But the former seems to have arrived at the verdict that the father was guilty primarily on the basis of the psychiatrists' subjective views expressed in the case-notes.

§611. When the case originally started, Ursula was 13 years old. And she was definitely mentally ill. Her first claims about having been sexually abused, emerged while she was admitted to a hospital. It is kept a strict secret whether the girl got the idea on her own; whether the idea was suggested to her by some psychotherapist; or whether strong pressure was applied to force it upon her.

Her first claim was that some unknown male had abused her at a single occasion. Later, when she was living with her parents, she accused a neighbour. But it turned out that he had a perfect alibi for the postulated time. We have seen from the cases of Betsy and Embla that neither judges nor prosecutors may take much impression of an alibi. Most of them are inclined to apply the most far-fetched ad hoc constructions to save the conclusion that the suspect is guilty. But in the present case the prosecutor did accept the innocence of the neighbour.

§612. Ursula had meanwhile reached the age of discretion, and the neighbour reported her for having falsely charged him with a crime. A police investigation was initiated, whence the girl was in a difficult situation. After two hours of police interrogation she got herself out of the corner by switching to the claim that it

was really her father who had abused her. And he had done so frequently for years, in the family house.

Considering the specific jobs, working places and working hours of both parents, as well as the time-budget of the family, the father had very little opportunity to be alone with the daughter in the house.

§613. Nonetheless, no less than 21 psychiatrists and other experts worked with the task of fabricating pseudo-evidence. The girl's conspicuous inclination to fabricate on sexual abuse would seem to constitute a major reason to be skeptical about the incest allegation. But this inclination was transmuted into the *major* proof against the father. By means of psychoanalytic theory the following ad hoc construction was invented, and was under oath claimed to be a frequent phenomenon. An incest victim may on the one hand want to expose the abuse, but on the other hand to protect her father. The result will be a *compromise formation*, whereby the victim may expose the abuse as such, while pointing out some different person as the offender.

§614. But if it were true that this pattern is frequent in the real world, the pattern could hardly fail to be present also in the incest literature. But so far I have been unable to find it there. Should the main psychiatrist claim that the pattern derived from her clinical experience (*and* from the non-clinical experience by a non-clinical expert of the team), the main psychiatrist could hardly have failed to be involved in quite a few comparable cases at the local district court.

The judges who convicted Ursula's father cannot be excused for having accepted the variety of evidence just described.

§615. I have presented only a few details from the case of Ursula. The account of the case of 19-year-old Rachel will also be brief. Her mother was definitely the originator of the incest allegation, which emerged after the divorce. But she and Rachel seem to have collaborated, *inter alia* with the purpose of taking over the father's house. They jointly made two reports against the father: sexual abuse of the daughter and illegal threat against the mother. They have eventually testified that these reports derived from nothing but benevolent motives. They thought the father was in need of psychiatric care, but had not been able to persuade him to seek help. Now they thought that his mental health would be improved by a long prison sentence. The judges of the district court accepted that Rachel had this motive.

§616. Allegedly, the father had performed oral and genital sex when Rachel was 11 to 14 years old, in the family house in Sofiatorp. But on a certain night the father saw a TV program on incest, and was shocked. He apologized for what he had done, and the abuse came to an end.

Now, the objective facts are that the family moved to Sofiatorp almost two years later than the postulated start of the abuse. Moreover, the TV program (which I have identified) was shown one year before the abuse allegedly stopped.

If she had told the truth, Rachel would bite and kick during the assaults. However, if the assaults took place at all, they were performed in an old creaking wooden house. And Rachel's own bed was in such a condition that it would creak

loudly at the least movement. There was no door from her room to the corridor, and her brother was sleeping—always with the door open to the corridor—in the immediately adjacent room.

§617. The following section of Rachel's account aptly illustrates the deficient reality feeling of the fabulator. She claimed that her father wanted her to buy condoms when she was 12 years old. He feared a suspicion of incest if he bought them himself, because everyone in the village knew that his wife used birth pills.

But he could most easily have bought condoms from an automat in a nearby town. And if he bought them in the village, his neighbours might at most suspect him of having a mistress.

By contrast, a 12-year-old girl who did not leave to her partner to provide condoms, might be suspected of considerable promiscuity. And if this suspicion seemed to be groundless, the neighbours might indeed speculate about other possible explanations.

§618. In a sense, the following pattern is the most informative in the case. When a person is reported for sexual abuse, the police will usually inquire about all varieties of nakedness in the family. And perfectly normal behaviour, in which the judges, the police officers, the prosecutor, and the psychiatrists frequently engage themselves, may be used to prove that the defendant is guilty of the accusation. In case after case, the perfectly innocent suspect may unsuspectingly supply such pseudo-proofs.

Rachel's father recalled that he had washed the daughter's whole body when she was 11 years old. The police interrogator informed Rachel about this event. But he also fabricated that the father had confessed that he had performed a sexual assault at this occasion. However, Rachel had no recollection of the event at all. But she discussed the latter with her mother, who realized certain possibilities. And then Rachel returned to the police and claimed to recall that her father had indeed abused her when washing her.

§619. At the present moment I am in a difficult position, because those facts which are appropriate from the purely scientific point of view, cannot be expressed without transgressing the code of legitimate academic language. It is an indisputable fact that there exists such a thing as a *logic of gossip*, which comprises a set of rather concrete rules for inventing false versions, for distorting authentic occurrences into false descriptions, and for deducing pejorative motives. (Apart from the principle of similarity, no feature is as prominent in Freud's patient descriptions, interpretations, and theories, as their root in the logic of gossip. I shall devote a future volume to the analysis of this aspect.) A few comments will be presented in ch. 53.

The district court wrote in the judgement that it is impossible that the father would have recalled the bathing event, if nothing more than washing the daughter had happened at this occasion. This is a *typical* derivation by means of the logic of gossip.

§620. The father was convicted by the district court. Because of circumstances which are not relevant in the present context, he was given a prison sen-



tence of "only" 18 months. The subsequent history of the case is however so illuminating, that an extensive presentation is called for. This presentation will follow neither the logical nor the chronological order.

For half a dozen years every Swedish judgement in incest cases has been based upon *the principle of the inverse burden of proof*. To escape a conviction the defendant must prove his innocence. To achieve a conviction the prosecutor need not even produce weak indications of his guilt. But I am not sure that any Swedish judgement can be found in which this principle is manifested in such a demonstrative way, as in the one written by the Court of Appeal in the case of Rachel.

§621. There seems to be a reason for this. The new defense counsel did an excellent job, and conclusively proved that Rachel and her mother had not told the truth. Being possessed by the aim of convicting the father, the judges seemed to have felt that they had to construct many arguments to explain away the evidence. Hence, the judgement is replete with logical acrobatics and ad hoc ideas. An example: although neither Rachel nor the prosecutor had suggested anything of the kind, the judges freely invented the fiction that the sexual assaults might have been performed in the morning before any other member of the family woke up.—Despite all such logical licences, the Court of Appeal has proven nothing except its own inability to produce any sensible justification for the conviction.

§622. In the district court Rachel had claimed that the father performed coitus interruptus *in her bed*, whence the semen would be spilled out on her stomach or in the bed. But the (second) defense counsel found out that Rachel's bed was in such a condition that the creaking would be heard over the entire house. Rachel therefore changed her version. She and her father laid a blanket on the floor. And the assaults were performed on this blanket. She would voluntarily go to the blanket. But having arrived there, she would fight and bite and kick. It may also have happened that her father found her sleeping in her bed and carried her on his arms to the blanket, so that she woke up on the blanket (!)

§623. Four witnesses testified that the mother had tried to engage them on her side in the trial, and two of them testified that the mother had tried to bribe them into committing perjury to have the father convicted of additional non-sexual crimes. According to Swedish law, the Court is not entitled to convict Rachel's father on the ground that one *cannot know* whether these four witnesses told the truth. A conviction requires that the Court *knows beyond any reasonable doubt* that they had lied.

But these testimonies are important because a Norwegian girl, Inez, had been located, who had occasionally lived with the family many years before. She testified that she had masturbated the father some 10 times when she was 13–14 years old. She was asked who else slept in the same room as her. Rachel and her older brother did. Had she masturbated while two other persons were sleeping next to her and the father? She seemed surprised at the question, but answered that she and the father went to *the corridor* (!) where the acts were performed. One could not easily imagine a place in a house that would be less fitting for such acts.

§624. *The deficient reality feeling* is a typical characteristic of fabulators. Very often they are unable to imagine what *additional* features of the situation or the event would also have pertained, if the postulated ones were autentic. Rachel's account is replete with the same kind of oddities as the example just mentioned from Inez's account. The Court of Appeal had noticed these oddities, but had tried to explain them away by means of these words

"However, when it is a matter of crimes of the variety at hand, it may be a far from unusual occurrence that the offender is prepared to run such risks." [Q-624:1]

Indisputably, an empirical generalization is asserted in Q-624:1. Equally indisputably, any empirical generalization must be *established*. The present one cannot be derived from ordinary lay experience. It seems that its basis could only consist of scientific research results, or of the judges' own inductive experience from previous cases.

A passing remark. For some 10 years both mass media and [pseudo-] academic papers have extensively disseminated the view that sexual abuse of children is a variety of *secret criminality*, which therefore went unnoticed for centuries. And in trial after trial, *including the present one*, courts have arrived at the verdict that no evidential power can be attributed to the fact that no other family member saw, heard or suspected anything, because sexual offenders are careful to conceal the offense. To this date I have in the literature encountered a total of 6 cases in which there is a postulation of the contrahends having been caught in flagrante delictu.

Hence, it is no little surprise to find Q-624:1 in a judgement.

§625. The judgement was unanimously passed by three judicial judges and two lay judges. I asked them what kind of a warrant they would invoke to justify Q-624:1. One judicial judge was on holiday, and one lay judge absolutely refused to make any comment. But the answers of the remaining ones are so extraordinary that a comprehensive presentation is called for.

§626. The chairman admitted that the generalization of Q-624:1 was not based on any scientific research results. It was based on "her experience as a judge". I asked her: if I procure the exhaustive set of the judgements of all incest cases she had handled in G-town, would I find this pattern in any of these? She answered that not everything is explicitly stated in judgements, *and (!)* that she had also been a judge in other towns. I made the question more precise: If I procure all *the documents* of such cases of hers from the last two years, would I find this pattern? She was not sure that I would, but she *believed* that I would. I went on: should I interpret her words so that she did not exclude the possibility that she had not at all encountered the pattern in any case during the last two years? She answered that *if I phrase the question in this way*, THEN she does exclude this possibility.

§627. The most revealing answer was given by the judge who had the main responsibility of the case. He frankly admitted that Q-624:1 is not based on any scientific results. As for his personal experience, *he was not sure that he had ever*

encountered the postulated pattern in any previous case. He said that *all* judges had made this “assessment” (!) “We have made this assessment”. And he invoked as support “the general experience we have got from various quarters”. He stressed that the court had not claimed that the generalization *is* true, but merely that the latter *may* be true.

The last statement implies a certain amount of dishonesty. A strong assertion about a “FAR FROM UNUSUAL” pattern cannot legitimately be defended by reference to a hidden reservation that it is merely *possible* that the strong assertion is true. Moreover, it is a recurrent *stylistic* feature of jurisprudential language to use slightly tentative formulations which are conceived of definite assertions. This feature is unambiguously present in Q-624:1. The generalization is applied as a *necessary* link in a chain of arguments used to establish that there *is* nothing inherently implausible about Rachel’s claims; *not* that it is a *mere possibility* that certain factual circumstances do exist by means of which the glaring implausibilities in her account *might* be explained away. It would be an illegal act to send a defendant to prison on the basis of a *mere possibility*.

§628. The lay judge was also asked on what ground she considered Q-624:1 to be true. She answered: “This is my view”. And she went on to say that it is her position that she completely agrees with the formulation. I proceeded to ask what is the basis of this view of hers. She answered that she had been a lay judge for 4 years. Asked how many incest cases she had handled during these 4 years, she said, 4 or 5.—Had she observed the pattern of Q-624:1 in 3 or 4 of these cases? She could not tell, but she was sure that she *had* encountered the pattern. I proceeded to ask: if I procure all judgements in cases of sexual abuse in which she had been a lay judge, would I find this pattern? She was not sure of this, since not everything is explicitly written in the judgements. But if I procure all *documents* in these cases, would I find the pattern? She could not tell, but said that she had also learned about other cases which had been presented to her.—Were these cases presented to her during the deliberations of the judges after a trial? [This question is important because, if so, the presentations must necessarily have been excessively brief.] She answered that she is forbidden to supply any information about what goes on during the deliberations. Had she taken any courses for lay judges on sexual abuse? She said she had not, and I concluded that she could only have encountered the cases during the deliberations. She said that it *need* not be so. She *could* have studied cases on her own, out of a personal interest. I asked how many such incest cases she had studied. She said she could not tell. Could it be more or less than 10? Once more, she said she could not tell.

§629. The picture is clear-cut. The judges used an empirical generalization to convict a defendant. They did not have the slightest ground for believing that this generalization exists at all. (There is no reason to waste space upon the hypothesis that those two judges who were not interviewed, were in the possession of genuine knowledge which they imparted to the others. If this had been true, the three judges would have expressed themselves in quite different ways.)

The judges were prepared to send a man to jail on the basis of a “fact” con-

structed out of thin air, and they were perfectly aware of what they did. During the interviews they applied the strategic rule, "Be evasive, but never tell a direct lie except as the last resort." All of them finally had to use the last resort.

§630. The judges might have fabricated the generalization during the trial at hand. But there exists a plausible alternative hypothesis. They may have borrowed the idea from newspaper accounts of the Swedish cutting-up trial. I shall discuss this source. The relevant information could be presented in a few lines. But most of my readers have altogether erroneous ideas as to the nature of the facts of this case, and might see the relevant information in the light of these ideas. I feel it is mandatory to describe certain aspects of the case. The reader should also be warned from the start: the case itself is so extremely surrealistic, that it is a sheer impossibility to give a true account which does not look like an evil-minded parody.

The two defendants, an autopsist and a general practitioner, will be given the names Dr. Laurence Autonne and Dr. Emil Gendel. The latter's daughter will be named Henriette. There is no reason to give a pseudonym to the deceased prostitute because her real name, Catrine da Costa, is well-known in Sweden.

§631. In 1984 Mrs. Gendel suspected her husband of having sexually abused the one-year-old Henriette. If she told the truth, the only ground for her suspicion was that she thought she could see that the hymen was broken. She took the daughter to a gynecologist who, however, found the hymen intact and also found every other feature of the child's sex organ perfectly normal. Mrs. Gendel was not satisfied with this result and had Henriette examined by a total of 4 gynecologists, all of whom confirmed the same result.

Mrs. Gendel always disliked Autonne, the former teacher of her husband. When she read in a newspaper that Catrine da Costa's corpse had been found partitioned in several parts, the idea occurred to her that Autonne and her husband had murdered the prostitute and performed a perverse sexual desecration of the corpse. She reported them to the police. *Without her intervention, Autonne and Gendel could not easily have been suspected.* There is no evidence at all that they had had any relation to each other for years when Catrine died.\* There is no evidence that any of them knew her or has had any contact with her. There is no evidence that the corpse has ever been at the institute for forensic medicine where Autonne was employed. There is no evidence that she was murdered. There is no evidence that the cutting-up of her corpse was performed with any other aim than to conceal the corpse; the 28-year-old girl was a heroine addict, and if she had died in a place associated with a bootlegger or with one of those doctors who is known to have supplied her with drugs, he would have had a motive for disposing of the corpse. Finally there is no evidence that the cutting-up of the corpse was performed by anyone having undergone medical training.

§632. The last statement needs certain comments, which will later turn out to have very important consequences. The seventh vertebra of the corpse was intact. And all experts agree that only a highly skilled surgeon would manage to separate the head between the sixth and the seventh vertebrae without injuring the lat-

ter. But some of the experts add that there is no evidence that this is what was done. The head was never found. Hence, the head might have been separated between the fifth and the sixth vertebrae, and both might have been broken. And *this* could well be the reason why the seventh vertebra was unimpaired.

All other parts of the partitioning are a butcher's job which any layman could have performed.

§633. The testimonies of prostitutes and narcotic addicts who have merely repeated what was already in the newspapers cannot be taken seriously. Basically, the evidence consists of two categories: psychoanalytic interpretations, and eyewitness identification. On the basis of the former category, Autonne and Gendel were convicted of murder and sexual desecration by the district court (one out of 7 judges voted for acquittal at the first trial). In a way which is unique in Sweden, the case has wandered for 7 years through 6 different courts, which have produced 10 judgements or verdicts. Apart from the first one, the most important is the ninth, produced by the Administrative Court of Appeal. This court completely rejected the evidential power of the psychoanalytic interpretations. It did so because of the critical examination by two witness psychologists, Astrid Holgerson and Birgit Hellbom (1991). Their report is a scientific masterpiece, which should definitely be published in an English translation.

§634. However, during the intervening 7 years an extensive and intensive campaign for a conviction had been performed by mass media, the feminist organizations, *The National Board of Health and Welfare*, and *The Psychological Association*. The Administrative Court of Appeal had also received 547 lists signed by some 6000–25000 persons, demanding a conviction. Enormous demonstrations were arranged by the feminist organizations outside the Administrative Court of Appeal, with placards about "Jack the Ripper". Hanna Olsson (1990) wrote a bestseller named *Catrine and Justice*. It is not a rhetorical expression but the appropriate descriptive formulation from the scientific point of view, that Olsson has mixed up trivial truths with the most infamous mendacity, applying exactly the same technique which may be observed in the Anti-Semitic writings by Josef Goebbels and his co-workers.

Few judges have the courage to oppose such a nation-wide craze. But to justify a conviction, the Administrative Court had to find some other pretext. And it found the latter in the eyewitness identification. The two doctors were convicted of having performed the sexual desecration of the corpse, and were deprived of their medical certificates. (The period for prosecution had expired, and the Administrative Court of Appeal does not handle criminal trials.)

§635. A photographer had repeatedly developed medical films for The Carolinian Institute. He recalled a particularly horrifying film in which a corpse was partitioned. In the beginning he could not state the year of this event (hence it may well have occurred several years before Catrine da Costa died). And he could not tell whether the corpse was male or female. But he did recall two features; and if his testimony in this respect be taken seriously, the corpse simply *cannot* be this prostitute. He claimed to have noticed that the leg was amputated



at the knee in a particularly tidy way, and he said there were bruises on the corpse.

At the line-up he identified two different men, being certain to 70% that one of these was the man who handed in the film. None of these men is even remotely similar to Dr. Gendel. But in the Administrative Court of Appeal he was absolutely certain that Dr. Gendel was the man, and that the event occurred some two weeks after Catrine da Costa was seen practicing her profession for the last time. He also claimed not to be so old that he could not distinguish a female body from a male body.—The photographer's wife likewise identified two different men at the line-up (but not the same as her husband), and was in the court equally certain that Gendel was the man.

§636. Judges and jurors have a superstitious faith in eyewitness testimony. In an American case the jurors convicted a defendant of rape on the basis of the eyewitness identification by the victim, although the DNA analysis performed by FBI conclusively proved that the defendant could not possibly be the person who had deposited the semen in the victim's sex organ (Doyle & Loftus, 1990:2). Another case is known in which a woman was raped while looking at television. She transposed the face she had seen on TV to the rapist. (Doyle & Loftus, 1987: 96).

Such instances are commonplace. They are not pathological aberrations. They are fundamental constituents of the *normal* function of human memory. People are very seldom in a situation in which much depends upon whether their memory is exact. Hence, they have very little opportunity to learn how poor their memory really is. This is true also of judges and jurors. Despite a comprehensive body of facts firmly established by psychological research, judges and jurors usually adhere to what may aptly be termed "flat earth psychology". Evidently, they are in urgent need of assistance by witness psychologists.

§637. The Administrative Court of Appeal completely accepted the final testimony by the photographer and his wife. Thereby, the crime had been pinned on to Gendel. The next problem was to pin the crime on to Autonne. Ignoring the clear-cut facts stated in §632, the Court concluded that the head had been separated by a highly skilled surgeon, while the legs etc. had been cut off by a person lacking surgical skill. The Court also ignored the photographer's testimony of the tidy amputation of the knee. Out of thin air the idea was constructed that, since Gendel is not a trained autopsist, he could do no better than the butcher's job observed on the corpse. Hence, he must have been assisted by a more proficient doctor. And the Court asserted that the assisting person could only have been Autonne.

§638. Next, a few specimens of the psychoanalytic interpretations will be presented. The interpretations were made by Frank Lindblad, a psychoanalyst and child psychiatrist who is generally considered the foremost expert on sexual abuse of children connected with *The National Board of Health and Welfare*.

In the Scandinavian languages the fatty stains in soup may be called "eyes". It is not known whether this circumstance is relevant. But Mrs. Gendel reported



that Henriette had said while eating soup: "One can eat eyes, and Daddy's eyes have the best taste."

According to Lindblad, this statement proves that Henriette (at the age of 17 months) was present while her father and his former teacher performed the sexual desecration, whereby they eat the eyes of the corpse. In two trials in the district court, 13 judges out of 14 have accepted this variety of evidence, while 6 of them even found it satisfactory for a conviction for murder of an individual who may have died from a wide variety of natural causes.

§639. Mass media and many academicians have misrepresented the conflict between Lindblad and Holgerson as referring to whether one should be open-minded and accept Henriette's testimony made years later, or should reject her statements on account of the apriori prejudice that a 17-month-old child cannot have noticed such things. But the indisputable fact is that Henriette has supplied no testimony at all.

Mrs. Gendel has tape-recorded many of her conversations with her daughter. She is clearly seen to behave like a schoolteacher trying to teach the child certain lessons. Evidently, Henriette had no recollections to rely on, and she misunderstood things. One instance will be described.

§640. In Scandinavia, Christianity mingles with Pre-Christian folklore. Santa Claus is named "the Christmas Brownie". I have chosen "Laurence" as the first name of Dr. Autonne to achieve a certain similarity with "Uncle Claus". Henriette had a book on brownies and was evidently very familiar with its content. The mother initiated numerous dialogues about "Uncle Claus", and the child responded. The mother imagined that both of them were talking of Henriette's experiences with Laurence Autonne. But the daughter has never given any sign of conceiving of "Uncle Claus" as a real person. Nor is it very probable that a 2-year-old child would say, "He was *so* little and tiny" about an adult man.

After some pressure, Mrs. Gendel had made Henriette state that she had visited "Uncle Claus" in his home. The mother continued: who else was there? Clearly, she was fishing for "Catrine da Costa". But Henriette answered, "Granny".—Recall 5-year-old Synnöve's statement in §§559f. that she had fucked her 7-year-old brother, while Daddy had fucked Granny.

Dr. Lindblad drove Henriette past Dr. Autonne's house, and also took her to the autopsy hall at the forensic institute where Autonne worked. Henriette gave no sign of recognizing these places. Lindblad testified in the courts that the child gave clear signs of having had such painful experiences at these places that she had "*repressed*" everything. (An analogous pattern is encountered in §§717–722.)

§641. Actually, Lindblad saw Henriette for no more than some 120 minutes in toto, and most of the encounters took only a few minutes. But during the period of his examination he participated in protracted meetings each week with the prosecutor, the police and a forensic pathologist, to learn what kinds of things they wanted him to "prove".

When the prosecutor needed proof that Autonne and Gendel had *murdered*

Catrine da Costa, Lindblad's data conclusively proved that Henriette had been an eyewitness of both the murder and the subsequent desecration of the corpse. When the prosecutor was forced to drop the accusation of murder, Lindblad's proofs of the desecration would have looked strange if they had still proved the murder. Without gathering any new data and without performing any new analysis of the available data, Lindblad made a volte-face. Now the very same data only proved the desecration and gave no hint of Henriette having been present at any murder.

§642. We need go back at least 25 years, to the great Swedish cases of judicial corruption (Unman, Kejne, Enbom, Selling) to find anything of a comparable nature.

In general, it is not very easy to learn what kinds of reasoning judges really apply. Those deductions which actually led to the conviction or the acquittal may appear so strange that the judges prefer to substitute them with others. At the present stage of scientific knowledge it is therefore not inapposite to bring forth anecdotal evidence. One judge of an appellation court, who has not been involved in the cutting-up case and whose knowledge of the case derives from mass media, nonetheless considered Autonne guilty. And he supplied two reasons. Autonne was said to look "uncanny"; and he had performed an autopsy upon the mother of this judge.

We do not know how many verdicts are actually based upon this kind of deductions.

§643. Now finally come the few lines which are relevant for the case of Rachel.

According to the prosecutors and 13 judges of the district court, the desecration of Catrine da Costa's corpse was performed on Whit-Monday 1984 in the autopsy hall at the institute of forensic medicine in Solna (a suburb of Stockholm). Autonne and Gendel have pointed out the glaring implausibility of this construction. The risk of being caught in flagrante delictu would be very great, since numerous people might unexpectedly turn up.

Frank Lindblad has rejected this argument. In the court he testified that *the high risk of exposure may instead have given an extra thrill to the act*.

When the Court of Appeal asserted Q-424:1 in the case of Rachel, its assertion may well be a plagiarism of Lindblad's testimony. If so, we may safely conclude that the Court took the idea from a newspaper report, since the number of documents in the cutting-up trial are gigantic, and Lindblad's idea is not easy to find unless one is prepared to devote an excessive amount of time to the study of these documents.

Only two alternative hypotheses are worth considering. The judges could have fabricated the empirical generalization themselves. Or a minority of the judges could have *heard about* the generalization in some *informal* context.

§644. Much more need be said about the case of Rachel. The Court of Appeal strongly rejected the view which (as we may read in the judgement) had been presented "in the debate" (!) that defendants are convicted solely on the basis of

the testimony of the alleged victim. On the contrary: they are convicted because the Court considered the facts of the case, and thereby made the assessment that the alleged victim is trustworthy. I may remark in passing that any Swedish court will almost invariably arrive at the "assessment" that the alleged victim is trustworthy, regardless of the nature of the facts of the case.

§645. Nonetheless, the Court produced a list of indicators which are said to reveal whether the alleged victim had told the truth. The list is not claimed to be exhaustive. But I shall cite the latter in toto.

- IND-1 The defendant's behaviour, way of recounting, or appearance in other respects.
- IND-2 The versions supplied by the defendant at different occasions (viz. in police interrogations or in different courts) may reveal that new information is added, or that previous information is changed. The versions may contain manifest exaggerations or directly false information.
- IND-3 The versions supplied by the alleged victim at different occasions may reveal that new information is added, or that previous information is changed. The versions may contain manifest exaggerations or directly false information.
- IND-4 The capacity of the alleged victim for describing the details of the alleged crime.
- IND-5 The capacity of the alleged victim for relating the alleged crime to specific events.
- IND-6 The view of other persons as regards the trustworthiness of the alleged victim.
- IND-7 The view of other persons as regards the trustworthiness of the defendant.
- IND-8 Observations of a general nature which may have been made by other persons at the time of the alleged crime.
- IND-9 The alleged victim may have shown physical or psychic problems which may be connected with the alleged crime. [Q-645:1]

§646. All the indicators are either completely invalid, or they are invalid in the sense in which they are taken by the court. It is a myth that certain kinds of physical or psychic problems are associated with sexual abuse. And judges, jurors, psychiatrists, clinical psychologists, laymen, and any person who is not trained in scientific procedures for assessing trustworthiness, are simply not capable of such assessment. Moreover, it is hardly possible to assess the trustworthiness of *individuals*, while it may be profitable to assess the trustworthiness of *statements*.

The overwhelming majority of fabrications derive from two and only two sources. Either, they are *modifications of authentic occurrences*; or else, they are borrowed from a *standard set of fabrications*. As for fabrications belonging to the former category, it will *by definition* be a matter of routine to relate them to specific events.

And many fabrications are rich in details.

§647. As a textual analyst, I am highly familiar with the second and third indicators. They will however yield no valid results, unless scientific analytic procedures be applied. And few judges are acquainted with such procedures.

Furthermore, most judges of Courts of Appeal are singularly disinclined to compare versions supplied at different occasions by the alleged victim. The prevailing policy is to base the verdict upon the oral accounts presented at the ongoing trial. As we shall see in §677: in the case of Ingalisa the Court of Appeal did not even permit the defense to present certain hard evidence about what Ingalisa had said on previous occasions. And the justification was that she was present in the courtroom and, hence, might just as well state things there.

§648. Five of the indicators are defined in relation to the victim, 3 to the defendant, and 1 to other people. This is a very unbalanced distribution. It seems that the Court attributes considerable evidential power to a certain feature if the latter belongs to the account supplied by *the victim*—but no evidential power at all if it belongs to the account supplied by *the defendant*. What if both accounts are very detailed and have undergone no modification on different occasions?

Moreover, the alleged victim will have plenty of time to fabricate a detailed story. Usually, the defendant cannot say much more than that such things did not happen. While he *can* produce alternative details, the court will usually claim they are irrelevant.

§649. The Court of Appeal also claims that the passage of time will favour the defendant, because the alleged victim may no longer recall all details of the crime. It is not even possible that the judges are in good faith. Most judges are prepared to invent the most extreme logical acrobatics to save the account of the alleged victim. By contrast, the defendant may be unable to prove what he did at the time in question.

§650. To document this attitude, I shall present the case of Hildegard, an adult narcotic addict. To get money for drugs she reported her father, a policeman, for having performed certain sexual assaults many years earlier. She learned that the period for prosecution had expired. Then Hildegard returned and reported her father for having forced her to sleep with him when she was 14. She could definitely date the rape to May 1986, because she had an abortion two months later.

It turned out that the father had a perfect alibi for the entire month of May. Then Hildegard changed her version once more, claiming that the rape had taken place at some unspecified time during winter or spring. The father was convicted by both the district court and the Court of Appeal, while Hildegard got damages of 100 000 Swedish crowns.

After this, Hildegard and her boyfriend, who is also a narcotic addict, boasted to other addicts about how they had procured money for drugs. Some of the others reported the matter to the police. The Supreme Court has therefore decided that the case should rest until these reports have been decided upon. Should the prosecutor or the courts find that the evidence is not strong enough to *convict*

Hildegard and her boyfriend, the Supreme Court may well decide that no evidence has been produced which may throw any doubt as to the guilt of Hildegard's father.

§651. Usually, a father or a stepfather accused of sexual abuse 6 years previously, cannot recall what he did at the time. Even if he does recall a pattern of circumstances which would constitute a perfect alibi, those people with whom he was together may no longer recall the time of the event. A noticeable asymmetry exists. Judges do not excuse a deficient memory of defendants or their witnesses. But they easily excuse a deficient memory of an alleged victim. For instance, Rachel originally claimed that the abuse had occurred at a certain place during a certain period. When it turned out that the family lived in this place for only a part of this period, she preserved the time but changed the place. But her third and final version was that the place was correct while the time had to be changed.

§652. The Court of Appeal admitted that Rachel's claim that her father wanted her to buy condoms (cf. §617), is indeed strange. Nonetheless, the Court saved the claim by means of the following deduction. Rachel had ALSO stated that her older brother used to have condoms in his wallet, and that it was a recurrent joke in the family that Rachel might steal condoms from him. Her father has confirmed both items. But I guess it was not a recurrent joke when Rachel was 12 years old; the judges completely missed this point. Instead, they concluded: THE LATTER information does not give the impression of being fabricated. And BECAUSE of the authenticity of THE LATTER information, Rachel LIKEWISE told the truth about her father's attempt at making her buy condoms when she was 12.

The reader may himself try to show that the Court arrived at this conclusion by applying the indicators listed in Q-645:1. He may do the same thing as regards the other deductions advanced by the judges.

§653. Both Rachel and her mother made a sizable number of changes of their versions between the proceedings in the two courts. Except when the claims were specifically related to the alleged assaults, the mother and the daughter made the very same changes. The Court of Appeal concluded that the mother may well have stimulated Rachel to tell the truth, rather than to keep the truth to herself. But there is no indication that the mother had in any respect influenced the content of Rachel's account. Cf. the third indicator in Q-645:1.

§654. The Court could not deny the zealous activity of the ex-wife of having the father accused also of a number of other crimes. But the judges concluded that the mother was so altruistic that she, three years after she had got another partner, wanted to help the father to get adequate psychological treatment.

§655. Without any sensible argument, and hardly with any argument at all, the Court concluded that Inez had told the truth. "Nothing has emerged at the interrogation or elsewhere which may support any suspicion that she has not supplied correct information". The reader may try to apply this statement to §633.—The Court strongly emphasized that the discrepancy between the accounts given by Inez and Rachel's father clearly reveals that the father is lying. The implica-



tion is that, "since" he "is" lying about this "fact", he may lie about everything. I construe this argument to be an application of the second indicator: "The versions of the defendant may contain directly false information."

§656. Swedish courts do not tape-record the account of the defendant. As a result, there is no hard evidence as to what the father said about the bathing event in the district court. But in the Court of Appeal he supplied a version which was confirmed by the mother. (Recall from §§618f. the deduction of the district court.) The mother had spilled out milk upon Rachel, and the girl had gone mad with rage. The father had tried to calm her down by washing her.

If a child is mad with rage and the mother is in a more or less adjacent room, the situation may not be very suitable for performing coarse sexual assaults. The judges completely overlooked this fact.

§657. Rachel was 11 years old at the time of the bathing event. She was 20 when she was asked by the police about the event for the first time. And she had no recollection of it then. Half a year later, in the district court, she could recall that her father had washed her "a little carelessly" and had touched her breast. In the Court of Appeal, when she was 23, she recounted that the father had masturbated upon her at the bathing event, had exhibited his penis, and had tried to place her hand upon his penis.

§658. Rachel explained the discrepancy between her versions by means of two factors. *She had previously not been asked any question about those circumstances she had added.* Or she had gradually recalled more details of the events.

The Court of Appeal found both explanations convincing.

Some readers might instead recall from §§442ff, the tendency of many fabrications of becoming progressively more extreme, comprehensive, and daring, as time goes by.

§659. The attempt by the Court of Appeal at refuting the argument that Rachel had hardly said anything on her own in the district court, but had merely given her consent to the statements presented by the prosecutor, is noteworthy:

"In this context there is reason also to comment upon the view of [Rachel's father] as regards the interrogation of Rachel by the prosecutor in the district court. By and large, these views may be summed up: [the father] thinks that Rachel actually did not supply very spontaneous information; that a much too large number of questions were so formulated that she merely had to answer yes or no; and that the answers were often put into her mouth. As far as the Court of Appeal can find, there is in no essential respect any support of this criticism. Admittedly, Rachel was often asked questions which induced her to answer either yes or no. But many of these are follow-up questions, where the aim of the prosecutor seems to have been to confirm that he had correctly understood information she had already supplied. Furthermore, the transcription reveals that parts of the tape-recordings were inaudible, a circumstance which will produce difficulties when the task is to draw conclusions in retrospect about both the questions and the answers made during the interrogation. The Court of Appeal, for one, thinks that the content of the phonographic transcription does not warrant critical views of the variety advanced by the defendant." [Q-659:1]

§660. Because of space considerations I can only discuss a few of the flaws in this excerpt. The repeated phrase "yes or no" seems to be a persuasive stratagem



aiming at giving the erroneous impression that Rachel *either* confirmed or denied the versions suggested by the prosecutor. But "No" may often imply consent, such as in the following example: "You didn't want to suck him?" "No."

§661. *The argument of the Court will collapse if spelled out completely*, because then it will take the following form. Rachel's contributions to the interrogation in the district court may be divided into answers to follow-up questions and answers to other questions. While a high frequency of answers of the *former* variety may indeed consist of no more than "Yes" or "No", the answers of the *latter* variety have an altogether different character. Consequently, the frequency of "Yes" and "No" does not prove that the total set of Rachel's statements contains hardly any information.

The logical structure of the spelled-out argument is sound. But its empirical premises are manifestly wrong. If the argument had not been partially concealed, many readers might have wondered what kind of people judges are. Since Rachel said "Yes" or (much less frequently) "No" to *all* varieties of questions, it is trivially true that she INTER ALIA did so to many follow-up questions. Moreover, a juxtaposition of each and all Rachel's 330 answers was available to the Court. Hence, the Court was perfectly aware of the emptiness of her entire contribution.

§662. As for the inaudible parts of the interrogation, 288 of Rachel's 330 answers did *not* contain any inaudible fragments. No less than 117 completely identified answers consisted of no more than the word "Yes", while there are only 5 answers with inaudible fragments in which "Yes" is the only *identified* word. No less than 34 complete answers consist of no more than the word "No", while there is only 1 answer with inaudible fragments in which "No" is the only identified word. Twelve additional complete answers are of the variety "Yes this is true", "I don't know" etc. No less than 163 of the completely identified answers (= 57%) are devoid of *any* information supplied by Rachel herself.

*The judges were aware of these facts.*

§663. Because the above analysis was available to the Court of Appeal about one year before the proceedings, it was also available to the prosecutor, to Rachel, and to her mother. It may or may not be true that the ex-wife deliberately tried to ruin the life of her former husband. But if it is true, any sensible person would predict that the mother would see to it that Rachel did not commit the same flaw in the Court of Appeal. But the judges attributed genuine evidential power to the fact that Rachel's own contributions in the Court of Appeal were more noticeable.

§664. A psychiatrist had met the father on one single occasion and for a total of 3 hours. In her affidavit to the Court of Appeal she explicitly wrote that the father was indisputably guilty and was permeated with perverse sexuality. She used psychoanalytic theory to heap insults upon the defendant, emphasizing "his" reality distorting defense mechanisms and "his" inability to face his own nature.

Before the first trial in the district court another psychiatrist had performed another psychiatric examination and had produced an affidavit. The latter was much

less explicit. Nonetheless, the first psychiatrist included a strange claim: the fact that Rachel's father expected a conviction constitutes a reason for believing that he is guilty.

§665. Throughout the present report I have repeatedly illustrated the low degree of knowledge of human nature observed among numerous psychiatrists and clinical psychologists. The father might not have known about the irresponsibility of most judges. But he was amply confronted with three circumstances: the cynicism and zeal of his former wife in furthering her plot; the almost total passivity of his first defense counsel; and the violation of Swedish law by the prosecutor, who refused to gather any evidence which could not be used or misused to facilitate a conviction.

§666. The father's second counsel requested the police to perform certain additional interrogations. The prosecutor was forced to have them performed. But he managed to avoid having the appropriate questions asked, and his personnel indulged in irrelevant topics. From some of these interrogations one could never guess that the accusation was about a sexual offense; one would rather think of economic criminality.

Even the Court of Appeal had to admit the poor quality of the investigation by the prosecutor. But the Court revealed its bias by suggesting that this poor quality was the fault of the defendant. He had no training in police investigations, and he was plagued by a somatic illness and by the intrigues of his ex-wife. Nonetheless, the Court seemed to imply that it was *his* obligation to suggest what data the police might search for.

§667. I have devoted more space to the case of Rachel than to any of the others. The reason is not that the judges were less competent or less honest than those who handled the other cases. The crucial point is that the judges felt forced to justify their verdict in a more explicit way. As a result, they have in the most transparent way exhibited their inability to produce any sensible justification. They have also demonstrated, even more than the judges who convicted Embla's father and those who refused a new trial to Betsy's father, their willingness to send a man to prison whom they *know* is innocent.

§668. Reflecting upon the nature of the reasoning of the judges, the reader will easily recognize the psychoanalytic methodology. This does not mean that they have learned this methodology from psychoanalysts. As I shall show in a future volume (but have briefly touched upon here and there in the first two volumes), the psychoanalytic standard operation procedure, the principle of similarity, and the illusion of separation, are no innovations. Freud learned them from primitive gossip ideology, and he adopted them because he himself had a primitive personality permeated with this ideology (cf. *inter alia* ch.53). The judges could easily have borrowed the methods just mentioned from the same source. Anyway, they took the father's guilt as an *apriori* axiom, picked up certain trivial details, fabricated evidence out of thin air when needed, and ignored or explained away clear-cut counter evidence by means of an idiosyncratic logic.

§669. Among all cases I have come across, only Embla, Hildegard and Ingal-

isa are known for certain to have invented false incest allegations on their own initiative. And Embla merely gave in to a momentous impulse, apparently without having given a thought to the possibility that her father might be harmed.

By contrast, 17-year-old Ingalisa was permeated by hatred of her stepfather. She also had much difficulty in getting along with other people. Her relation to her biological father was excellent at the time she made the incest allegation and moved from the stepfather to the biological father, but this relation soon deteriorated.

§670. Immediately before midsummer 19.. her biological father assisted her in going secretly to her cousin for a summer holiday. When her mother and stepfather got to know where she was, they told her to return immediately, thinking with no little justice that the cousin had a bad influence. The conflict lead to her moving permanently to her biological father. In July she reported the stepfather for incestuous assaults. Allegedly, the abuse started when she started school, and *stopped 1 March* (note this date). Despite the claim that the abuse had ceased since four months, she also claimed to know that it would continue if she should return *home*.

§671. During the police interrogation she was at first surprised at having stated the date, 1 March, and suggested that she had been mistaken.

But later during the interrogation, she spontaneously recalled the date, and told that a quite different event had occurred at that time. Her stepfather had called one of her schoolmates an idiot. And the girls had discussed whether to report him to the police because of an illegal insult. Note how Ingalisa mechanically transposed a central constituent of the insult incident to the allegation of sexual abuse.

§672. At the initiative of the prosecutor a pseudo-witness-psychologist was appointed. I shall name him "Axlund", and he is an assistant professor of psychology. He was selected by the chairman of the district court. I asked him about his reasons for selecting this man. He answered that it was important that the task be performed by a person who has been trained in witness psychology *and* has also clinical experience with children. I have already pointed out that the training Axlund has undergone has nothing to do with witness psychology. But even formally he had no clinical experience at all with children.

Unsurprisingly, Axlund concluded that the stepfather was guilty. One of his main proofs is illuminative. Ingalisa had stated that her stepfather had touched her breasts on the outside of her clothes; and that he had also licked her sex organ. But allegedly, the former variant had exclusively been performed in her own room, and the latter variant exclusively in her parents' sleeping room. And this distribution of the kinds of variants over the kinds of rooms, reveals that Ingalisa had most probably told the truth.

§673. I shall disregard the fact that the distribution just mentioned has no root in Ingalisa's actual statements. Instead, I shall focus upon Axlund's deductive logic. One of his criteria is this "*differentiation*" of the postulated acts over various external circumstances. Like Dr. Paulus, Axlund does not realize that he has

constructed a formula which any future fabulator may apply. His stratagem is even superior to the psychiatrist's proofs, since it is hard if not impossible to find any fabulated version which is entirely undifferentiated (all kinds of acts took place in all rooms at all times of the days and while the persons had put on or off all varieties of clothes, and so on). The criterion might at face value look like a tool for distinguishing between true and false incest allegations. But in practice the criterion will lead to the inclusion of each and all allegations into the category of the true ones.

§674. Although other versions also exist, Ingalisa usually claimed that her stepfather had licked her sex organ *thrice*. She did not resist when he removed her pants for the first time, because she had no idea as to what was about to happen. Neither did she resist at the second or third occasion, because she had meanwhile forgotten what he did last time he removed her pants.

But this account is contradicted by a quite different one, according to which Ingalisa and the stepfather had made an agreement: in exchange for his licking the sex organ the girl should receive some favour, probably a permission to stay out for a longer time.

How could these two account be reconciliated? Did the stepfather and the stepdaughter first negotiate an agreement, whereafter a blackout befell Ingalisa so that she was truly ignorant of the aim of uncovering her private parts?

Or did the stepfather first undress her without having so far given any hint as to the aim—but when his mouth approached her sex organ she stopped him and demanded a counter contribution?

Few judges seem to be capable of detecting this variety of inconsistency.

§675. The stepdaughter claimed not to have any knowledge as to whether her stepfather had masturbated while licking her—because, as she said, in order to find out, she would have had to raise her head and take a look.

She repeatedly assured that both were *lying in the bed* during the acts. But she was totally incapable of describing the positions. The police interrogator eventually suggested that she might draw the positions on paper. And then Ingalisa asked for pictures to assist her in drawing.

She was instead given dolls. After experimenting with the dolls for a non-neglectible amount of time, she arrived at the conclusion that the stepfather must have been *standing on his knees on the floor*.

§676. The entire narrative of the girl is permeated with this formal pattern. Manifestly, the allegation consists of nothing but *verbal phrases*. The latter have no foundation in any authentic recollections—nor even in any fantasies. If she had at least had fantasies to rely upon, she could not possibly have been at such a loss, whenever she was asked to supply a minimum of concrete content to the abstract phrases.

§677. In the district court the prosecutor wanted to use the video-taped and audio-taped police interrogations as evidence of the stepfather's guilt. They were permitted. In the Court of Appeal the defense council wanted to use the very same tapes as evidence of the stepfather's innocence. They were forbidden.

The stepfather was sent to prison for 8 months. Apart from purely formal comments, the judgement of the Court of Appeal comprises 46 words. This is a clear signal that the court was uninterested in anything the defendant or his council might say.

§678. One further element of the case is noteworthy. The leader of the Witness Psychological Laboratory, Astrid Holgerson, did not analyze the facts of the case, but she did analyze Axlund's investigation. She clearly showed that Axlund had not really investigated anything at all and, hence, was not entitled to draw any conclusion. The district court sent both her and Axlund's pronouncements to *The National Board of Health and Welfare* for an evaluation. And the evaluation by the Board is definitely illuminative.

The objective facts are that neither Axlund nor Holgerson have any clinical experience with children, and that both of them have performed numerous previous evaluations of the trustworthiness of children in incest trials. In both these respects no difference can be found between them.

§679. *The National Board of Health and Welfare* completely rejected Holgerson's pronouncement, with the justification that it is always altogether unacceptable that such investigations be performed by persons who have had no clinical experience with children.

A few lines later the Board stated that Axlund's pronouncement is satisfactory in every respect, because Axlund has the previous experience of evaluating the trustworthiness of children in incest trials.

## The Cases of Reger, Cynthia, and Vessela

Prosecutor:

*Did he at any occasion insert his fingers into your vagina?*

Lucinda (17 years old):

*I don't know. I didn't look at him.*

Prosecutor:

*Well, but you might have felt it?*

Lucinda:

*I don't know.*

Police interrogator:

*Was there nothing of his pressing you down and keeping hold of your hands? Was there a great tumult?*

Lucinda:

*Almost every time he definitely had to keep a firm hold of me.*

Interrogator:

*Could you ever make yourself free by kicking him?*

Lucinda:

*I wouldn't dare do that either.*

Interrogator:

*You say yourself that you didn't want to. You didn't want to. Isn't that so?*

Lucinda:

*He might have understood things as if I wanted to, how should I know.*

*The nature of Lucinda's account very strongly points towards her having experienced what she has recounted. The district court rules that the decision of the question of guilt of the accused should be based on Lucinda's account.*

Kjell Björklund & Jerker Lundén &  
Gunnar Åreskog & Kurt Johansson & Åke  
Avedal & Ingemar Andersson

§680. The case of Mr. Reger is well-known in Sweden, and I shall give only a brief summary.

The foremost expert of *The National Board of Health and Welfare* on somatic signs of sexual abuse, will be named Vinborg. This assistant professor is very prone to take anal marks caused by constipation as clear indications of anal assaults. (She has done so in the case of Vanessa, other features of which will be discussed in ch. 34.) Two cases are known to me, in which she retracted her testi-



mony *during the trial*, after genuine experts had pointed out that no crime had been committed. But genuine experts are not often available.

§681. One of these two cases is that of Reger. Vinborg found proof of sexual abuse in his three children. A witness psychologist, who will be named Herning, was given the task of identifying the guilty individual among three possible candidates. Herning uncritically accepted Vinborg's postulation that the children had indeed been abused. And she pointed out Reger. The district court sentenced him to prison for 8 years without any evidence other than the testimonies of these two expert witnesses. The Court of Appeal reduced the sentence to 6 years.

§682. After Reger had been arrested for more than a year, a highly competent pediatrician explained the true etiology of the somatic signs. Because of her contribution, Vinborg admitted that she had made a mistake. In turn, Herning admitted that she had likewise made a mistake. She made a new investigation according to which Reger was innocent. A highly competent witness psychologist and a pseudo-witness-psychologist (Axlund) agreed on the innocence of the father.

Reger was eventually acquitted. He received damages of 700 000 Swedish crowns (a small sum for a ruined life).

Detective inspector Monica Dahlström-Lannes is considered the foremost expert on sexual abuse of children within the Swedish police. Her real qualifications as well as the nature of her morals are aptly illustrated by the fact that she has recently (1992) claimed that the greatest scandal in Sweden is that Reger was not convicted on the basis of the pattern of evidence described above.

§683. (Digression.) This is the only case I have been able to find, in which a pseudo-witness-psychologist has found the suspect innocent. Note however that Axlund made his investigation after all the other experts, and at a time when it was obvious that Reger would be acquitted. By making a verdict opposite to the one the court would soon make, Axlund could only have undermined his authority in future cases.

Furthermore: *judges* are not thought to be compromised when they send dozens or hundreds of innocent people to jail. The same is true of *psychiatrists* and *clinical psychologists*. And the prevalent view in Sweden is that Vinborg is not in the least compromised by her handling of the case of Reger. All blame is laid at the door of Herning, whose proportion of erroneous verdicts is conspicuously much less than that of Vinborg.

§684. Much additional information may emerge if two other cases, those of Cynthia and Vessela, are seen in the light of the case of Reger. I shall hardly discuss the content of the case of Cynthia. Instead, I shall focus upon a few crucial aspects of an important new case-law passed by the Supreme Court in July 1992. Certain parallel features between the cases of Ursula and Cynthia are noteworthy. Both girls originally claimed to have been raped by unknown males. Both of them later claimed to have made up these assaults. Both of them eventually stated that their fathers had forced them to submit to numerous acts of sexual intercourse.

Finally, in both cases one or more experts testified that the following pattern is

far from unusual. A girl may have two conflicting inclinations: to reveal her incestuous experiences, and to protect her father. The result may often be a Freudian compromise solution (it is irrelevant that the experts did not use this terminology): the girl will report the assaults, but she will point out the wrong offender.—Needless to say, this theory has no foundation in the real world. It seems to have been specifically constructed for the purpose of facilitating false convictions.

§685. The new case-law in the case of Cynthia was formulated or accepted by 5 judges: Vängby, Heuman, Magnusson, L.K. Beckman and L.Å. Beckman. Three of these have previously made erroneous verdicts in the cases of Betsy and Violet. In the judgement at hand they rejected the investigation which was made by the same witness psychologist who had helped the Court to correct the false conviction of Reger. But the Supreme Court emphasized that no court should automatically accept the conclusion by an expert witness. The court should make an independent assessment as to whether the conclusion is really substantiated by the facts of the case.

There is no sound reason for presenting this idea as an innovation. From the very start and continually for some 35 years the witness psychologists themselves have propagated exactly this view about the relation between experts and judges.

§686. I am no witness psychologist. But I have repeatedly pointed out that courts at all level neglect their obligation of evaluating the claims of *clinicians*. We shall see in a moment that the judges who passed the judgement in the case of Cynthia transgressed their own principle.

They stated a number of specific reasons for rejecting the investigation by the witness psychologist in the case of Cynthia. I shall not discuss these reasons. It may suffice to say that they are based upon the same variety of flat earth psychology which was revealed by the Court of Appeal who convicted Rachel's father.

§687. Two expert witnesses sided with the prosecutor, although they had formally been appointed by the court. One was Vinborg. The Supreme Court has had ample opportunity to observe her low competence. The other was a school psychologist, who will therefore be named "Schulbaum". The latter testified that Cynthia was trustworthy both when she accused her father and when she retracted her previous claim about having been raped by unknown males. It was also Schulbaum who asserted the explanation involving the Freudian compromise solution.

§688. One may search in vain through the judgement for a sign of the Supreme Court having made any attempt at evaluating Schulbaum's surrealist theory. The formulations reveal that the court has mechanically and with hardly any awareness adopted the theory.

The judges' demonstrative neglect of a principle which they strongly emphasized makes it hard to escape the conclusion that this principle was not intended to be taken seriously. It seems to be a strategic pseudo-argument aimed at rejecting evidence favouring the defendant, while preserving pseudo-evidence supporting the position of the prosecutor.

§689. One more crucial point. According to the new case-law, the defendant should not really be permitted to engage an expert witness. The justification of this suggestion is that such an expert witness could not easily be objective, while an expert witness appointed by the court would have a much greater chance of being objective and independent of both the prosecutor and the defendant. Moreover, there would generally be no reason to appoint more than one witness psychologist, although there may be reason to appoint also a clinical child psychologist or a child psychiatrist.

Strictly speaking, a Swedish court cannot easily prevent an expert witness from testifying. But the court may decide in advance not to pay any attention to the testimony of an expert engaged by the defense.

§690. To this date I have never come across one single case in which a child psychologist or a child psychiatrist appointed by the court, did not arrive at the conclusion that the defendant was guilty. We may safely assume that the experience of the Supreme Court is not very different from mine.

§691. But it is known for certain that the judges were perfectly aware of the following facts. Reger was innocent. He was convicted on the basis of very poor investigations by two experts who were appointed by the court. His innocence was proved by two other expert witnesses who were engaged by the defendant. *The new case-law will completely prevent the correction of such absurd convictions.* There can be little doubt that *this is the secret aim of the Supreme Court.*

The experience in Sweden is clear-cut. Expert witnesses engaged by the defendant are very careful to preserve their integrity as scientists. Their investigations will usually hold water. In cases of sexual abuse, expert witnesses appointed by courts are of a mixed quality. But in the majority of cases they have forged evidence in order to assist the prosecutor. Prosecutors are much more acquainted with the degree of competence and honesty of different experts, and may at an early stage influence the court's selection. Assisting the prosecutor may therefore lead to many future appointments.

§692. I shall anticipate a relevant element from the case of Vessela. In the latter, Herning made one more incompetent investigation. The district court mechanically adopted Herning's conclusion and convicted Vessela's father.—How would the Supreme Court treat a new trial motion? A number of highly qualified jurists agree that the Supreme Court would never make a critical evaluation as to whether Herning's conclusion be substantiated by the facts. The *only* way for the father to be acquitted, would be that Herning retracted her conclusion. In that case the Supreme Court would mechanically and without any independent assessment adopt Herning's later standpoint.

§693. To sum up the case of Cynthia: the automatic acceptance of the postulations of the clinical psychologist; the suggestion that a witness psychologist should always be appointed by the court (and it is secretly understood that he should be selected from the list produced by *The National Board of Health and Welfare*); the suggestion that one witness psychologist is sufficient; and the suggestion that no attention should be paid to the testimony of an expert witness en-

gaged by the defendant; all these ideas have a common denominator. They will remove the last obstacles to the conviction of flagrant innocent fathers, and they will make false convictions irreversible. For a number of years *The National Board of Health and Welfare* has fought for exactly these ideas. It is a dangerous sign that the Supreme Court has joined this fight.

§694. A brief postscript may be appropriate. The Supreme Court invoked 6 papers in support of its view. One may doubt the wisdom of founding a major legal precedent upon articles in semi-popular periodicals. Moreover, the judges have made no critical evaluation of these papers, nor of the context in which they are embedded. Could the selection of the papers have anything to do with editorial policy?

No less than two papers were written by Frank Lindblad (1991) (1992), some of whose interpretations about Henriette were cited in §§638 and 640. Lindblad's first paper was jointly published together with Monica Dahlström-Lannes (1991) and Norelius (1991). Dahlström-Lannes's ideas as regards the cases of Reger and Vessela are presented in §§682f. and 697. Norelius is the pseudo-witness-psychologist of §556 whose report led to the arrest of Carola's father. It is evident that all three papers were produced in collaboration. Although the Supreme Court did not invoke all three papers, the entire pattern should have given rise to second thoughts.

One of the papers invoked is Sven-Åke Christianson (1992). He predominantly discusses the trial of Christer Petterson for the murder of Prime Minister Olof Palme. I may remark in passing that Petterson should never have been tried. But he was rightly acquitted thanks to the investigation by Holgerson.—Christianson also attacks Holgerson's contributions in the cutting-up trial and in cases of sexual abuse of children.

§695. His article is an inconsistent mixture of, on the one hand, scientific purism: he wants to restrict the role of the *non-clinical* (!) psychologist in judicial trials to carrying out experiments and presenting experimental results. He even neglects the problem of the generalization of such results to the kinds of situations encountered in the court. On the other hand, Christianson reveals a degree of carelessness incompatible with a scientific attitude. Conspicuously, he is unacquainted with the documents of the cases he pretends to analyze, and has used secondary sources of a doubtful quality. Although the documents reveal the very opposite state of thing, Christianson states that Holgerson is unacquainted with the Devlin report except through secondary sources. (Apparently, the Supreme Court was strongly influenced by this disinformation.)

He advances a set of genuine scientific research results *against* Holgerson. Despite the biased selection, these results strongly support her conclusion. Nonetheless, he claims that her method and conclusions have nothing to do with science. And he recommends that she should not be permitted to function as an expert witness. (The reader may recall from the ingress to ch. 7 how Carl-Erik Brattemo attacked my doctoral thesis without having read it.) His primary argument may easily be applied to the eyewitness identification in the cutting-up trial, cf. §§636f.

§696. The facts of the case, the relevant scientific results, and the application of the latter to the former, seem clear-cut. But Christianson formulates the rule that only a psychologist who has himself performed experiments on human perception, should be permitted to apply any result established by the psychology of perception, to judicial cases. Isn't this view analogous to the rule that only he who has been in Spain (although not necessarily in Barcelona) should be considered competent to have any view as to whether *La Sagrada Familia* has more than one tower?

If Christianson is taken at face value, he wants to restrict the contributions of *non-clinical* psychologists to two tasks. Either, they may lecture on *general facts*. Or else, they may perform experiments on behalf of the court, with witnesses, defendants or victims.—It would be easy to test whether the photographer of the cutting-up trial could correctly identify individuals, *if he were forewarned* about the nature of the task. But how could we learn from the outcome of such a design whether his identification of Dr. Gendel was founded in any authentic recollection?

No less than thrice Christianson invokes "facts" borrowed from a newspaper journalist, Karin Thunberg. I feel myself unable to believe that Christianson is not perfectly aware that Thunberg deliberately distorts facts for the purpose of having innocent suspects sent to prison for sexual abuse of children. It seems that Christianson does the same thing. Unsurprisingly, Christianson, Thunberg, Dahlström-Lannes, Norelius and Lindblad unanimously propagate the view that a (real or pseudo-) witness psychologist should be appointed only by the court, and should be selected from the list produced by *The National Board of Health and Welfare*.

§697. The case of Vessela, to which I shall now turn, has a prominent place in the disinformation campaign in mass media led by Monica Dahlström-Lannes during spring 1992. She has severely attacked the district court for not having sent Vessela's father to prison or—as an alternative variety of *punishment* (!)—to a mental hospital.

Martin and Iris were Non-European immigrants. When they divorced, a dispute emerged over the custody of their 6-year-old daughter Vessela. And the dispute was fought in many Non-European ways. I do not think Iris acted like a revengeful Scandinavian ex-wife; she might even have been in good faith—for instance, she might have been stirred up by the extensive mass media campaign going on at the time of the dispute.

§698. Vessela moved to the mother in February 19.. According to the affidavit of the social welfare agency, the idea occurred to the mother during this month that Martin had abused the daughter. The mother has confirmed this time indication in court. It is however noteworthy that she was totally incapable of stating any reason for this early suspicion. She claimed that the child made the very first hint about sexual abuse in June. And the detection emerged when Vessela, who belongs to a nation with a very strict moral code, disapproved of the mother being naked. A few weeks later the mother reported her suspicion to the social welfare agency, who made a police report after two further months.



§699. Herning was appointed to perform a witness psychological investigation. Both she and the police interrogator had a hard time because, although Martin occasionally denied the accusation, most of his answers were without rhyme or reason.

The following paragraph is not intended as a kind of an advocatory plea for Martin's innocence. It is my position that the dialogues with the child unambiguously reveal that some adult person (presumably her mother) had *taught* her to utter certain sentences about sexual abuse—sentences which she did not understand. Below, I shall analyze a few of these dialogues. But first I shall have to dispose of a number of other subjects. One of these is the suggestion by Herning and others, that Martin's peculiar behaviour points toward his being guilty. I find no clear-cut clues in the interrogations of Martin but, if anything, weak clues pointing in the direction of his being innocent.

§700. Consequently, it may be appropriate to supply a non-exhaustive list of etiological factors which might have been responsible for Martin's answers. First, he had a markedly shy personality, and was not in the best psychic condition. Second, it seems indisputable to me that he was *genuinely* shocked by the attribution to himself of what he considered an abominable crime. Third, he correctly perceived that the police interrogator was aiming at having him send to prison at all cost, and had little interest in whether he was actually guilty. And I, for one, think that he would indeed have been in jail today, if he had "played the game according to the conventional rules". For some years I have wondered at the willingness of other suspects to play their role as if they were confronted with honest prosecutors and judges, who were prepared to take any impression of the facts.

Fourth, in some less developed countries the inhabitants may have *learned* that the safest way to escape unjust sanctions when one has to interact with the police, is to avoid to commit oneself to any definite position—either by refusing to answer questions or, if this is not possible, by giving uninterpretable answers.

§701. It *could* be that Herning had arrived at a different result if Martin had "collaborated"; she is not an incest ideologist. On the other hand, her erroneous analysis of the child's statements may imply that it would have made little difference how Martin had reacted.

When the investigation was performed, Herning was about to leave the profession of witness psychology. And her investigation is replete with irregularities. Genuine witness psychologists are careful to preserve their independence in relation to the police and others. Strangely, in the present case the witness psychological interviews were performed at the police station, in the presence of two policemen and a police interpreter. The police interrogator even took part in the interviews of Martin.—Clearly, he had good reason to perceive Herning as just another policeman.

§702. Herning writes in her report that Martin's behaviour could not derive from the danger of the situation, since he showed the very same kind of behaviour "also in calm situations without any dramatic elements." But if the perceived



purpose of an interview is to send the interviewee to prison, I fail to grasp how any subsection of the interview could possibly qualify as being calm and undramatic.

§703. In so far as her conclusion is based upon the interview with Martin, one of Herning's strongest arguments is her interpretation of the following statement (which is expressed in poor Swedish): "Is there difficulties which a family—family with children—no matter whether I am guilty or something." Herning took these words as a partial admission of guilt. But it is more likely that Martin intended to communicate the following idea. It is not true that difficulties may arise after divorce in a family with children only if sexual abuse is involved. One is not entitled to conclude that, since there are indeed severe difficulties in the family of Vessela, then she must have been abused.

§704. In case after case a mother or a psychotherapist assure that the child recounted sexual assaults to their considerable surprise, and without their having exercised the slightest suggestive influence. In quite a few cases tape-recordings are available with the therapist, and occasionally also with the mother. And in case after case the tapes unambiguously reveal how the child was first told the entire version, and was then put under strong pressure to say that such things had really happened.

Herning committed the mistake of taking at face value the assurance of the mother, despite the absence of any objective support. She also missed the clear indications that Vessela repeated ideas which some adult person had forced upon her; a subject to be primarily discussed in ch. 33.

## The Assessments of Vessela by Three Clinicians, and the Case of Arne

*In a curious turnabout, those who claim most strongly that children must be believed don't believe the child when the child says no abuse occurred.*

Ralph Underwager & Hollida Wakefield

§705. Vessela's three psychotherapists will be named Valpsund, Astberg, and Engskov. The incest therapy was started two weeks before the mother's report to the social welfare agency. Astberg saw the child 12x2 hours spaced over 11 1/2 months. After a pause of 2 months Engskov took over. During 6 1/2 months she saw the mother alone 13 times, and both the mother and the child together only twice. She does not seem to have ever seen the child alone. Engskov's affidavit merely transmitted what the mother had told but, strangely, did not mention sexual abuse. Valpsund saw Vessela 5 times during the last months of Engskov's treatment.

§706. To begin with, I shall discuss certain features of Valpsund's affidavit. Regrettably, features of this variety are encountered in case after case. On the first page this experienced clinician explicitly stated:

"Since the aim of the investigation was not to assess whether Vessela has been exposed to sexual assaults, *no facts have emerged which indicate that any assaults have occurred.*" (italics added) [Q-706:1]

But on the following page Valpsund repeatedly referred to *the established fact* that Vessela had indeed been abused:

"[...] *Those sexual assaults to which she has been exposed have lead to her inability to state it in the investigation. My assessment is that the experiences have been so painful to her that she has denied those things which have happened.*" [...]

"It is likely that Vessela has reacted upon and been harmed by sexual assaults. Research reveals that children are harmed by sexual abuse, in particular if the offender is a father figure. When children have been exposed to sexual assaults it is important that they receive psychotherapeutic treatment as soon as possible. Otherwise, the risk is considerable that the child will at adult age *feel a deficient trust in persons around her* and a negatively coloured self-image will emerge, an image Vessela shows already now. Research has moreover revealed that children who have been exposed to sexual abuse may as adults have sexual difficulties and *may show immature behaviours in relation to their own children.* It is clear that sexual abuse may constitute a threat to the future development and health of the child." (italics added) [Q-706:2]

§707. Valpsund acted as a counter part to Abendstern (cf. §§576ff.). She had likewise taken the father's guilt as an apriori axiom, and openly demonstrated her want of interest in empirical circumstances.

When she talked of "immature behaviour in relation to their own children", it seems that she had in mind Alice Miller's speculative idea that the abused child will as an adult be prone to abuse her own children. Miller is another anti-empiricist, who uncritically adopted and slightly modified Freud's idea that children who are seduced by adults are prone to seduce younger children rather immediately.

As we have seen in §§545ff. Mrazek & Mrazek (1981) distorted Katan's (1973) quite different speculations to make them agree with Miller's postulations.

§708. Psychotherapy with girls who really or allegedly have been abused, may have a variety of negative consequences. Many therapists deliberately try to frighten them into believing that they will probably abuse their own children in turn. But other consequences may be much more harmful:

"It is both puzzling and ironic that those most vociferous in championing the welfare of children and the need to protect them remain oblivious to the possible harm of a mistaken adult belief that a child has been abused." (Underwager & Wakefield, 1990: xii) [Q-708:1]

"The child may come to believe events happened and are real that did not happen and are not real. This is an assault upon a developing child's ability to distinguish reality from unreality.

[...] When adults elicit preposterous accounts from children, fail to exercise any critical rationality, but instead lend credence to a child's improbable accounts, those adults are abandoning one of their fundamental responsibilities to children. Reinforcing and rewarding a child's fantasies as if they were true makes the world into a confusing and threatening place for the child." (Underwager & Wakefield, 1990:107) [Q-708:2]

"This training to believe in grotesque falsehood cannot fail to harm this child. It may result in permanent damage to her contact with reality. It may cause her to perceive herself always as a victim, helpless and dependent and unable to resist any intrusion. She has learned to regard unusual and deviant behavior, such as group orgies, urophilia and coprophilia, as almost routine daily events. She has been desensitized to horribly degrading behaviors, sexual perversions, and lack of privacy. Her capacity to engage in healthy and rewarding sexual relationships once she becomes an adult is likely to be permanently diminished." (Underwager & Wakefield, 1990:125) [Q-708:3]

There is no doubt that Vessela had indeed developed neurotic behaviours during the dispute over the custody. The reader will easily recognize *the illusion of separation* (cf. Q-186:1) in Valpsund's neglect of the possible etiological effect of this dispute. But instead of helping the child, incest therapy might cement "a deficient trust in persons around her".

§709. Twice in Q-706:2 Valpsund invoked that "research" had shown various things. Now, it is a recurrent phenomenon that

"some professionals do not read research literature but rely upon casual word of mouth or informal discussions". (Underwager & Wakefield, 1990:12) [Q-709:1]

I myself have questioned Valpsund about what writers or papers she had in mind, giving her a few weeks to retrieve her recollections. It turned out that she was completely unable to recall any single writer or paper.

§710. According to the affidavit, Vessela had told Valpsund that her father had "fucked" her. Valpsund was skeptical as to whether the child understood the meaning of this word, and her main conclusion was that Vessela had during five sessions said nothing about sexual abuse. She paid no attention to the discrepancy between the child's silence on this subject and her use of an adult expression she did not seem to understand. Did Vessela by some kind of a miraculous coincidence happen to hit upon the term that is appropriate from the adult point of view? Or had someone *taught* her to say "Daddy fucked me" (a sentence which to her might have had as much sense as "Daddy lobotomized my kidney")? If Valpsund is correct about Vessela not understanding the word "fuck", there seems to be no third alternative, and the former alternative is by far the less probable. Why then did Valpsund not ask whether the child's statements on sexual abuse to other persons, could not likewise have been stamped in by adults?

§711. The extensive analyses elsewhere in the present volume have established that the psychoanalytic defense mechanisms "repression" and "denial" have no legitimate place in science. Valpsund invoked them to explain away the fact (as she understood it) that Vessela had said nothing about sexual abuse. Among incest ideologists, this is a highly frequent ad hoc device for saving the apriori axiom. The case of *Arne*, to be described later, provides an even more lucid illustration.

§712. However, even if the psychoanalytic concepts had been valid per se, repression and denial could not be applied to the pattern at hand.

Supposedly, the abuse took place before, and stopped in, February. Allegedly, Vessela told her mother about the abuse in June. She also told her first therapist during nearly a year. It follows that she had not repressed her recollections during the ongoing abuse; nor during the subsequent 16 months. If the events lead to no amnesia during both these periods, then *why* should repression occur *later*? And *at what time* is she thought to have suffered repression?

§713. As can be seen from Astberg's affidavit, she likewise took the father's guilt as an apriori axiom, and picked up a few details to *decorate* this axiom. She had "not the least doubt at all" that the father had abused the child. In support of this conviction she invoked both Vessela's *behaviour* and her *recount*.

Now, her list of neurotic behaviours contains no item which would be surprising in a child caught in a severe dispute over custody. Astberg has mechanically applied the *illusion of separation*.

§714. As regards Vessela's recount, Astberg invoked a total of one fragment. We may therefore guess that she attributed particularly strong evidential power to this fragment:

"In addition, I shall quote from a dialogue between Vessela and the observing psychologist [indication of date = 8 months after the start of the treatment]: 'Daddy is bad—do you understand?—No, the observer answers. Vessela says she won't tell why Daddy is bad. The observer says she need not tell why Daddy is bad. Vessela then looks at the observer with big eyes, and then says speedily: Daddy has done those things one does with one's wife in the bed. Do you understand? The observer answers yes and Vessela immediately leaves the subject.'" [Q-714:1]

Astberg did not seem to realize that the reference to what one does with one's wife in the bed is a typical formulation from the perspective of an adult. A 6-year-old child would hardly have used it on her own—still less a child *victim*. I shall say more about this variety of formulations in ch. 33.

§715. Evidently, neither Valpsund nor Astberg was competent of assessing whether Vessela had been abused. Neither showed any interest in the empirical observations. And both started out with the dogmatic presupposition that the father was guilty.

The analysis of the case will be resumed in the following chapter. But since two clinicians have invoked the concept of repression to explain why Vessela said nothing about sexual abuse (except a few isolated sentences whose meaning she did not seem to understand), I shall first give an outline of the case of Arne, in which this ad hoc device played an unusually manifest role.

§716. Different dimensions of this case may be interesting to different readers. The case provides a particularly lucid illustration of the peculiar nature of the Swedish judicial system. The existence of two parallel and disconnected subsystems (civil/criminal and administrative) absolutely prevents the correction of certain kinds of erroneous judgements. Moreover, the social welfare agency may be willing to sacrifice the happiness of children to pursue other aims. Finally—and this may be the most relevant aspect in the present volume—the psychoanalytic concept of repression may be used as a strategic device for explaining away the absence of empirical support of an aprioristic presupposition.

§717. This is the only case in which I have not studied the original documents. I shall rely on, and stay very close to, the presentation given in TV (*Rekordmagasinet*, 1985) by Jan Guillou. No valid objection could be raised against this decision, since this investigatory journalist has during two decades amply proved his meticulous care in checking all facts.

One linguistic detail is central. A certain Swedish verb literally means "to shake". But adults would hardly ever use this word in any other sense than "to masturbate". Whenever "shake" is intended as the rendering of this Swedish word, I shall use the spelling "shayke".

§718. Because of purely private reasons a social worker disliked Arne. Inter alia, after divorcing his wife he had won a custody dispute as regards his daughter Catharina, against the recommendation of the social welfare agency.

The social worker reported to her colleagues what Arne's present wife Eva had allegedly told her:

"One day when Eva was alone at home with Catharina and Lisa, the girls were playing

with the cat. Eva was listening to their small talk, and perceived the word 'shayke'. She asked the girls not to use words they did not understand, and added that a boy is needed for shaying. The Catharina answered—together spontaneously, according to Eva, that 'I know this of course, because I have done this on Daddy several times'. Thereafter, she suddenly became very frightened, rushed to Eva, and urgently asked her not to tell anything to Daddy." [Q-718:1]

Q-718:1 constitutes the crucial evidence.

§719. Two social welfare secretaries started the investigation, which was kept secret from Arne. They contacted his ex-wife and told her the alleged facts. She was then inclined to think that she herself might have suspected something of the kind. She was offered the custody of Catharina, which she gratefully accepted.

So far, the social welfare secretaries knew about the event only through a tertiary source. They had to ask both the secondary source (Eva) and the primary source (Catharina).

Both Eva and Catharina denied that the entire event, apart from Catharina having used the word "shayke" in the literal sense and in a harmless context.

§720. Having gathered the facts, the social secretaries wrote the following report:

*"All experience shows that children hardly ever lie about sexual assaults. An investigation performed in the USA shows that only 2 out of 10 000 children who had been exposed to sexual abuse, had lied about the abuse. It is exceedingly important for the future development of Catharina that she should receive the help and support she needs to work through her experiences."* (italics added) [Q-720:1]

On the basis of this report, the social welfare agency removed Catharina from the home of her father, and placed her in the home of her mother.

§721. Such decisions must be confirmed rather soon by the county administrative court. A plea was sent to the latter:

*"All experience shows that children hardly ever lie about sexual assaults. An investigation performed in the USA shows that only 2 out of 10 000 children who had been exposed to sexual abuse, had lied about the abuse. Catharina has a marked trait of repressing painful events and subjects. We take for granted that a protracted period of child psychological clinical treatment is needed, before Catharina will be sufficiently mature and strong to talk about her experiences."* (italics added) [Q-721:2]

Here, it is explicitly stated that the aim of the psychotherapy is to force upon the child the arbitrary conviction that she had been abused: and that one expected the indoctrination process to be very time-consuming.

The judge realized that the entire plea consisted of nothing but slander. But he was voted down by the laymen of the court. The social welfare agency was given the authority of handling the case.

§722. The agency and the mother speedily sent another plea to the district court. The latter started:

*"All experience shows that children hardly ever lie about sexual assaults. An investigation performed in the USA shows that only 2 out of 10 000 children who had been exposed to sexual abuse, had lied about the abuse."* (italics added) [Q-722:1]



The district court gave the custody of the child to the mother.

§723. Arne appealed against the judgement of the administrative court to the Administrative Court of Appeal. However, the latter refused to consider the case, and gave a peculiar justification. Arne had no longer the custody of Catharina. Consequently, it was no longer his business whether Catharina was or was not under the authority of the social welfare agency.

§724. Eventually, the prosecutor decided not to try Arne, since he had obviously committed no crime.

Thereafter the social welfare agency decided that he was probably guilty after all. He was not permitted to meet Catharina except under the supervision of the agency. However, the agency claimed to have found it impossible to arrange such supervision, wherefore he could not see his daughter at all.

§725. *Without a century of psychoanalysis, the concept of repression would not have been ready at hand to carry out this plot.* But from now on the causal direction may be reversed. If the production of such disastrous consequences by means of psychoanalytic concepts be tolerated, numerous laymen and academicians may conclude that these concepts are indeed valid—simply because otherwise it would be unethical to apply them. I do not believe in the possibility of fighting the theory alone.

## The Tape-Recorded Dialogues With Vessela

*It is a greater disaster for a child to be believed, with all the consequences ensuing in the present context, when there is no factual basis for the allegation, than to be so strongly disbelieved that there will be no legal consequences for the suspect.*

*I feel worried and disheartened because all those who talk of the afflictions of the innocent convict never mention the disaster of the child who send the very same innocent persons to prison, and who will have to live with this knowledge for the rest of his or her life.*

Magne Raundalen

§726. The witness psychological interview with the child took place 12 1/2 months after the mother had reported her suspicion to the social welfare agency, 13 months after the child had started incest therapy with Astberg, 18 months after the suspicion had occurred to the mother for the first time, and likewise 18 months after the child had moved from the father to the mother. Hence, layers of subsequent distortion may have concealed Vessela's original version. It may not even be possible to retrieve the latter. But at the very least, precautions are needed before one is entitled to conclude that any statement is related to any authentic experiences of sexual abuse.

§727. Herning showed little awareness of this problem. In her set of hypotheses she did include the possibility that the incest allegation derived from the mother's suggestive influence. But she disposed of this hypothesis in a single sentence, merely stating that the evidence is insufficient for evaluating its truth value.

I shall eventually show that the evidence is clear-cut. Moreover, from both the scientific and the judicial point of view, this hypothesis must be *refuted* before there is room for the conclusion that Martin was guilty.

§728. In ch. 28 we have seen how Synnöve's mother and psychotherapist influenced her to say that her father had "fucked" her. They were partially successful; but Synnöve clearly revealed that she did not understand such descriptions, because she had no experiences of her own to rely upon. During the incest crazes in Jordan, Minnesota, in Cleveland, England, and in other places, the indoctrination of dozens of children had been sufficiently successful to lead to numerous arrests of innocent people, at least two of whom committed suicide. Underwager & Wakefield (1990) have published extensive tape-recorded dialogues of interrogations of preschool children. One child was asked the very same question 80

times. Another child was interrogated for a continual period of 10 1/2 hours. Children have been prevented from going to the toilet until they have confessed that their father or someone else abused them. Swedish cases which are not basically different are known to me.

§729. In one of the cases in Jordan, Judge Robert Klein wrote:

"What the children now know to be fact is what they have learned through the process of questioning over the span of time and under the circumstances of the investigation/therapy. [...] In other words, the children lack a present memory of the events from which they can testify. [...] Cross-examination could not effectively penetrate the wall of learned facts to reveal any real perception. [...] [The girls] have been led and taught by the adults to produce the hoped-for responses. [...] In fact, because the children now believe that such abuse occurred, they are unable to separate the facts from their learned experience and, consequently, their behavior is just the same as if they were abused. The abuse has become their reality. [...] They cannot qualify as witness. [...] They lack personal knowledge, because they have no memory which the court can be assured is their personal recollection of the 'event'." (quoted in Underwager & Wakefield, 1990:214) [Q- 729:1]

§730. One of Herning's primary arguments as to why Vessela's statements refer to authentic experiences, is that they are clothed in a language that is appropriate for her age level. Below, I shall show that the opposite is true. But what if they were?

It is known that the mother and the daughter used their native language when talking of sexual abuse. Many mothers may adapt their language to the age level of the child. And if Iris had used adult expressions, Vessela would have had no choice but to translate them into the kind of Swedish she mastered.

Training young children to produce false accounts may not invariably result in versions formulated in childish language, but will frequently do so. This is amply illustrated by the tape-recorded interrogations of 10 children aged 3-6, which are quoted in Underwager & Wakefield (1990).

§731. Next, two particularly illuminative excerpts from the dialogues with Vessela will be quoted. V = Vessela, H = Herning. Strangely, the police interrogator and a further policeman were present during the witness psychological interview, and both of them took an active part in the interrogation. The child's limited proficiency in the Swedish language poses the problem what errors should be preserved, as well as what English errors are equivalent.

V-1: When the mummies and the daddies are sleeping with each others, this my Daddy did to me.

H-2: Hm.

V-3: And then I got ill because of that.

H-4: And how did you feel, in what way were you ill? How?

V-5: What does that mean?

H-6: Well, how did you feel?

V-7: Then I felt, well, I was fine but I became a little red here and and my face and then I got mighty tired and, I do not recall what I got. But I was this way but my face was mighty red.

H-8: How comes it your face got red?

V-9: Well, because of this my Daddy had slept with me. Such things Daddies should not do with children.

[Q-731:1]

H-10: What was it he put right?

V-11: It was, it was her willie which he would put right, because it was a little bit, it didn't feel well, then I say that 'it doesn't feel well, you can do it somewhere else', not here or here, but you can, what is the name, I cannot say it.

H-12: No, you don't know the name in Swedish? Have you ever seen a lady or a man doing it?

V-13: No, not on me, not on children.

H-14: No, on each other then?

V-15: Yes.

H-16: Where have you seen it?

V-17: Not on a child but on an adult. They kiss.

H-18: They kiss?

V-19: Yes.

H-20: Have you also seen when they are sleeping together?

V-21: No, I have only seen in the underground, once in the underground I saw a, a, eh, one of, it was two girl and boy they, one of girl or boy, no it was the boy or the girl, I don't know, said that 'shall we do it', then they kissed in the underground, my Mummy didn't see.

H-22: No, only you.

V-23: Yes, when I was, it was, I was not yet four years old I was, I was 6 1/2 now.

[Q-731:2]

§732. I shall juxtapose three of Vessela's statements, dating them in terms of the number of months since the mother's report to the social welfare agency:

"Daddy has done those things one does with one's wife in the bed" (to Astberg, after 7 months)

"When the mummies and the daddies are sleeping with each others, this my Daddy did to me" (to Herning, after 12 months)

"Daddy fucked me" (to Valpsund, after 19 months).

[Q-732:1]

§733. The absence of details is conspicuous in all three statements. As we have seen, Valpsund took the general position that the girl had "*repressed*" any recollection of the abuse, and she meant that Vessela did not understand the meaning of the last sentence in Q-732:1. But Valpsund was on apriori grounds convinced that the child had been abused.

If the assaults really took place, and if the child repressed them, it seems awkward that she used an expression which, from the adult point of view, is in perfect agreement with the authentic state of things. *From where* did this expression derive? And if *this expression* derived from other sources than authentic experiences, *how* would Valpsund argue for the probability that *other expressions* did not likewise derive from other sources?

Despite Engskov having conducted therapy for 6 1/2 months, she did not at all mention sexual abuse in her affidavit. Presumably, Astberg quoted the instance

which in her eyes had the strongest evidential power. But none of the statements listed in Q-714:1 has any evidential power.

§734. The first two of these statements are flagrantly clothed in adult language. Preschool children hardly ever refer to coitus in terms of people sleeping with each others. And such an expression would be even more improbable in the mouth of a genuine victim of genital intercourse.

I think an apt analogy would be the following. At the time when corporal punishment was still permitted in schools, let a preschool child have been thoroughly caned by her father. Then we may try to imagine this child describing her experience in the following words: Daddy did to me what is done to bad boys at school.

§735. For the sake of the argument we shall suppose that Martin actually performed oral and genital assaults; and that these assaults had resulted in Vessela having got ill and very tired and having got a red face.

The latter part of the pattern constitutes a *causal proposition*. And few preschool children would have gained a sufficient capacity for abstraction to be able to detect such a causal connection.

§736. Neither would it be easy for a preschool child to make the observations themselves. In what situations will the face of a human person become red? And what does he *feel* in these situations?

*The person himself will feel that his face is burning* but not that it is red. *Other people will see that his face is red* but not that it is *burning*. Human beings eventually *learn* that the feeling of burning is closely associated with the appearance of redness, and they learn to *name* their internal feeling in terms of the external appearance. But this is a relation which must be *learned*. Six-year-old children will seldom have learned it. In addition, those experiences Vessela recounted at the age of 6, took place (if at all) at the age of 4 or 5.

§737. Furthermore, a red face is associated with an emotion *and* an external situation which produced this emotion. The attention of adults is usually absorbed by the situation to a much greater extent than by their internal state. And absorbing is very much more complete in preschool children. We should therefore expect a genuine incest victim to say much more about the offender's behaviour than about her own reactions.

It might be that Vessela pointed between her legs when in V-7 she said that she became a little red "here". But if the colour derived from sexual assaults, it is more probable that Vessela would have stated that she became sore.—Young children may be red from a variety of natural circumstances. We know also that the mother soon after Vessela had moved to her, drew the attention of a medical doctor, who made a routine examination, to the urinary area of the child, and expressed some vague suspicions. Since the mother questioned the child for a protracted period about oral and genital assaults, it is not a far-fetched guess that the mother may also have told Vessela that she was red.

§738. In a sense, V-9 in Q-731:1 is the most revealing of all statements. Six-year-olds do not on their own invent moral rules and evaluations such as "Such things Daddies should not do with their children".

To sum up so far, the incest allegation is flagrantly clothed in formulations that are altogether alien to the thinking of a preschool child. They could only have originated in an adult. It is difficult to imagine that the adult originator could be anyone else than the mother.

§739. There is little reason to devote as much space to the analysis of Q-731:2. Evidently, Vessela could not have truly recalled her age at the time of the underground event; but nothing hinges upon whether she is mistaken on this point.

V-11 is difficult to understand. Vessela quoted another statement she had allegedly made at least 18 months previously. Did she claim to have said to her father during an assault that the act did not feel well to her, wherefore she recommended him to perform the same kind of act upon another female instead? If this is the idea Vessela intended to communicate to Herning, we may safely conclude that she mechanically repeated ideas an adult had put into her head.

§740. Herning's question whether the child had seen adults do these things, was asked in a context where the father's (alleged) genital assaults were in focus. Vessela answered that she had *not* seen it being done upon children. But she *had* seen adults do it upon each others. And *then* she supplied the concrete example that adults kiss.

Children may leap from one subject to another, without much awareness. But Q-731:2 gives the impression that Vessela had little knowledge of the difference between kissing and coitus.

In V-21 she does deny that she has seen adults *sleeping together*, while she has seen them *kissing*. We should however beware of projecting our adult perspective onto children, and take for granted that they use the words in the adult sense. In view of Vessela's limited knowledge of sex, she might simply have meant that she had not seen adults kissing in the bed, while she had seen them kissing in the underground.

§741. In case after case judges have demonstrated their inability of detecting the most conspicuous indicators as regards the question of guilt. Martin was convicted. But because of psychiatric considerations he was not deprived of his freedom, but sentenced to a specific kind of legal supervision ("skyddstillsyn").



## Somatic Evidence of Sexual Abuse in the Cases of Enhagen and Vanessa, and the Need for an Office of Public Defense

*In some children "the assaults remain in the body".  
They cannot swallow soured milk (e.g.) without having strong nausea (from oral assaults).*

Monica Dahlström-Lannes

§742. Although the focus on sexual abuse started as an attempt at providing job facilities for underemployed psychodynamic therapists, the trend eventually has grown into a world-wide plague involving many professions; inter alia gynecologists and pediatricians.

A far from infrequent genetic variant is that girls are born almost without a hymen (Huffman & Dewhurst & Capraro, 1981). A young child may look as if a broom-stick had been pressed through the hymen. Enhagen's three daughters had this variant. Two chief physicians, in whom *The National Board of Health and Welfare* has a special confidence, reported these "signs of sexual abuse" to the police. Enhagen was arrested for one month, whereafter the cause was removed from the cause list.

§743. Although neither of these two doctors is a psychiatrist, it is easy to recognize in the affidavit by one of them the principle of similarity, the psychoanalytic standard operation procedure, and the illusion of separation, together with a poor degree of empathy and the lack of knowledge of human nature:

"A highly aberrant behaviour was observed in both children during a home visit. Inter alia, it was reported that both children [two girls aged 1 and 2] had been lying upon each other, making jogging movements with their abdomen. Because of this, suspicion is instilled that the children may have been exposed to sexual abuse." [Q-743:1]

§744. The case of 2-year-old Vanessa is much more tragic. She suffered from hypothyreosis. Her parents, Harry and Ilona, were political fugitives from a Non-European country. It is not known whether they had been frightened by mass media propagation, but they suspected that the syndrome might derive from sexual abuse at the day nursery. They jointly requested a gynecological examination. Unfortunately, the latter was performed by three incest ideologists, who forged a series of pseudo-proofs. Two of these doctors, both of whom committed perjury in the courts, will be named Dr. Meyerbeer and Dr. Enesco.—They are the same ones who were responsible for the arrest of Enhagen.

I shall not focus upon the entire case, but merely analyze one of the proofs. Allegedly, Vanessa had a vaginal orifice of 15 mm. And allegedly, the comprehensive literature which the doctors had collected over the years, agrees that this measure is much too large for such a young child. But although Méyerbeer and Enesco had access to the very same papers, the former set the limit at 10 mm, while the latter would not allow for a natural measure exceeding 4 mm.

§745. The first point to be noted is that 15 mm is the *stretched* measure (by the finger, by palpation). And I have been unable to find any stretched measure in the entire literature. The *unstretched* measure under anaesthesia was 10 mm. Dr. Meyerbeer testified that the anaesthesia could at most change the measure to a neglectible extent. She even supplied a theoretical explanation as to why this is so. Nonetheless: she herself had written in the case-notes that because of the anaesthesia it is impossible to decide whether 10 mm is too large a measure.

The unstretched measure without anaesthesia was 5–8 mm; presumably, the amplitude derives from the child's movements.

§746. The comprehensive literature collected over the years turned out to consist of a total of two brief papers, Cantwell (1983) and Berkowitz (1987). And in the latter no more is stated than that *some* writers *take* a measure exceeding 10 mm as an indication of sexual abuse.

By contrast, Cantwell claims to have established that 74% of those girls *up to and including 12 years of age*, who had a vaginal orifice greater than 4 mm, turned out to have been sexual abused. This figure tells more about Cantwell than about the experience of the girls. Huffman & Dewhurst & Capraro (1981) indicates 10 mm as the *mean* value of *normal* girls of 11 years of age, without stating how much larger a *normal* orifice might be. Moreover, a bewildering variety of measures may be found throughout the literature; a clear indication of the limitation of contemporary knowledge.

§747. Both these cases brings to the fore a fundamental question. *What patterns of facts constitute a proof, and what patterns constitute a sham proof or a pseudo-proof?*

A related problem should not be ignored. A prosecutor may have spent years in developing a network of experts, whom he can always trust to support his position in the court. (Abendstern, Axlund, Enesco, Leonard, Meyerbeer, Paulus, and the pseudo-witness-psychologist who investigated Carola, belong to such networks.) Generally, the defense council cannot start to look for an expert before he has a case. In the beginning, he must *guess* whether an expert could at all contribute to the improvement of the case. It might well turn out that there were universal agreement on Meyerbeer's and Enesco's claims about the vaginal orifice.

The defense council also need an expert who has a sufficiently strong academic position; sufficient courage to stand up against widespread but erroneous ideas; a capacity for making advanced scientific results understandable to laymen (e.g., judges or jurors); etc.

§748. Not the least important aspect is whether the expert is in the possession of the *relevant* knowledge. It is by no means obvious that the most competent ex-

pert on gynecology (as a science, a set of diagnostic procedures, and a body of therapeutic tools) may be best equipped for fighting pseudo-scientific peculiarities. As Martin Gardner (1957:11) writes in one of his books on crank science: "If someone announces that the moon is made of green cheese, the professional astronomer cannot be expected to climb down from his telescope and write a detailed refutation." But I wish to add that the professional astronomer might not even have the prerequisite knowledge for writing a detailed refutation of this variety of ideas.

§749. There seems to be only one solution to the problem: an office of public defense must be created, a counterpart to the office of public prosecution, likewise with genuine resources of performing investigations and gathering facts.

It has frequently been said that there is no need of such an office, because it is not the obligation of the defendant to prove his innocence but the obligation of the prosecutor to prove his guilt. This objection is invalid.

The cases described throughout the third book unambiguously reveal that judges at all levels are prepared to convict defendants despite the absence of any indication that they might have committed the crime for which they are tried—and even despite clear-cut evidence of their innocence. In cases of sexual abuse, the prosecutor is not asked to prove anything.

§750. The defect just mentioned of the judicial system could easily be remedied. It is much more difficult to come to grips with another problem. A pattern of circumstances—say, Vanessa's "enlarged" vaginal orifice—may look like a strong proof of sexual abuse; and may yet be nothing of the kind. It would be a matter of routine to present a long list of comparable patterns. Few judges are capable of assessing the significance of such evidence. But an office of public defense might have gathered the crucial information in advance of a particular case.

Moreover, prosecutors and psychiatrists might be less inclined to try to achieve convictions by means of pseudo-proofs which are equally easy to produce about guilty and innocent defendants, if they knew that the defense had a reasonable chance of exposing such devices.

## Concluding Reflexions

*There were neither witches nor bewitched until  
they were talked and written about.*

Alonso de Salazar Frias (1612)

§751. The cases described throughout the third book are in many respects dissimilar. They may give rise to a number of reflexions, some of which may at the first glance seem to bear little relation to the main subject of the present volume. Eight flagrantly innocent fathers or stepfathers were by the district court sentenced to a total of 33 years 8 months in prison. After subsequent acquittal or reduction of the sentence in two cases, a total punishment of 21 years 8 months remained. One father was sentenced to legal supervision. Among those who were not convicted, 4 had been arrested for a total of about one year, and 3 lost their children.

The contemporary pattern is in every non-trivial respect equivalent to the picking out of suspects at random, whereafter so-called experts may forge pseudo-evidence to justify a conviction.

§752. Serious harm may be done not only to the defendant, but also to the girls, who might not have been able to resist the considerable pressure to make a false report. These girls must live a whole life at the same time being aware of what they have done.

Those persons who most strongly talk of the necessity of helping the children or teenagers, are often least concerned about the harm done to them.

§753. Regardless of whether the objective of prisons is thought to be to punish or improve the prisoner, it must be strongly demoralizing to both the convicts and the staff to see a large proportion of innocent convicts. And such a proportion may interfere with the objective. At the Swedish prison to which sexual convicts are primarily sent the prison doctor, Thomas Eriksson (1991), has stressed the considerable increase of this proportion during the last half decade. He specifically referred to the cases of Betsy and Violet, without thinking they were exceptional.

An individual who has committed a crime may *understand* why he is punished, and may feel that the punishment is just. But it is a Kafka-like nightmare to have to spend years in jail, and eventually be released with a severe social stigma, if one has never transgressed the law, and if no honest man could fail to perceive his innocence at the trial.

§754. A study of the psychological processes of judges may be an invaluable

service to mankind. Do we know whether these processes are rational or irrational? Do judges take into consideration that it is profitable to their career to pass the kind of sentences requested by "the public opinion"? Or would a scientific study reveal the same variety of thinking by means of which Nordvall arrived at the conclusion that Odenmark was guilty (cf. §557).

§755. The incest ideologists perform intensive lobbying, in relation to both judges and journalists. What judges read in newspapers on sexual abuse in general may have a much greater impact upon their verdicts, than the facts of the concrete case going on in the courtroom.

Moreover, some judges attend courses in sexual abuse, lead by teachers explicitly acknowledged by *The National Board of Health and Welfare*. Even if the number of such judges are not great, they may impart to their colleagues the pseudo-knowledge they have learned.

§756. It would not be appropriate to excuse judges by the suggestion that they had been misled by psychiatrists. It is part of the judicial system to try to mislead judges. And it is the obligation of judges not to permit themselves to be misled. Besides, only a judge who was prepared to be misled, would take any impression of the kind of evidence exemplified throughout the third book.

Nonetheless, the present state of affairs has been brought about and is maintained by psychiatrists. If these psychiatrists should abandon the incest ideology, we may guess that the judges would follow suit.

It must be admitted that the majority of fully trained psychoanalysts disapprove of the arbitrary attribution of experiences of sexual assaults to patients. At the same time, these ideas are almost exclusively applied by people who adhere to psychoanalytic theories. This is true even if they belong to the professions of gynecology or pediatrics etc.

§757. It can be seen from the documents of concrete cases that judges apply no critical analyses to the contributions by psychiatrists and clinical psychologists. Judges look upon this variety of expert witnesses as a kind of oracle. In this respect they passively mirror certain unwarranted claims which these clinicians have extensively propagated during a whole century, viz. that they are in the possession of a unique capacity for "seeing through people" and "expose the ultimate reality".

§758. From the very beginning and until the very present day, psychoanalysts and psychoanalytically orientated psychotherapists have been insensitive to human suffering. They have always been prepared to sacrifice human happiness to pursue their own peculiar goals. For generations they prevented severely sick patients from obtaining adequate treatment. Still today some of them conduct hateful campaigns against pharmacological treatment of psychotics and behaviour therapy of neurotics. If most of them now show a partial tolerance of behaviour therapy, the reason is not that they have eventually become more responsible, but that they have no longer any choice.

I have repeatedly stressed the surprising continuity of psychoanalysis. The very same cynical attitude is manifested in their endeavour to send thousands of

flagrantly innocent individuals to prison, in order to promote their own prestige and economy.

Robert Emans has emphasized the responsibility of the psychological and psychiatric profession for the state of things, and has called for scientific protests and refutations:

"Techniques often associated with psychological science play a prominent part in the process that results in many of the false accusations. [...] The psychological profession's inability, or unwillingness, to speak out against the misuse of unscientifically based instruments in child abuse may be a partial reason why portions of the public may question the value of psychology." (Emans, 1988:1000) [Q-758:1]

§759. It is not a far-fetched hypothesis that the contemporary flourishing of the incest ideology must be understood as a desperate attempt to secure the survival of psychoanalysis, in a situation where its position within the field of therapy and understanding of psychopathological syndromes is rapidly declining. If this be so, one may welcome the approaching end of ideas which have done so much harm to science and humanity.

On the other hand, the last stage of psychoanalysis may have a brief *or* a very protracted duration. If psychoanalytic ideas are permitted to get a firm rooting within the field of sexual abuse, they may experience a renaissance and may flourish for another generation before the first signs of their waning may be noticed.

As if this were not enough: from this point they may significantly retard the decline within psychopathology. Many psychologists and psychiatrists may feel that the entire academic community would be severely compromised, if the activity of some of their colleagues were unethical pseudo-science. They may conclude that, if thousands of people can be sent to jail for many years on the basis of psychoanalytic interpretations; if very young and slightly older children can be forced to undergo incest therapy for years—a therapy which is *not* stopped by the most unambiguous signs that the children are severely harmed; and if enormous public expenditure can be devoted to the training of incest diagnosticians and incest therapists at the most esteemed universities; *then* there must be no little truth in psychoanalytic theory.

Consequently, the extensive discussion of incest trials is not an alien element in the present report. There is little prospect of cleaning away psychoanalysis from those fields it had usurped until about 1980, if we do not work as hard on the task of cleaning away the latter from the field it has usurped recently.



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- (GW-VII:203-209) = Charakter und Analerotik.
- (GW-VII:243-377) = Analyse der Phobie eines fünfjährigen Knaben.
- (GW-VII:379-463) = Bemerkungen über einen Fall von Zwangsneurose.
- (GW-VIII:93-102) = Die psychogene Sehstörung in psychoanalytischer Auffassung.
- (GW-VIII:239-320) = Psychoanalytische Bemerkungen über einen autobiographisch beschriebenen Fall von Paranoia (Dementia Paranoides)
- (GW-X:12-22) = Ein Traum als Beweismittel.
- (GW-X:44-113) = Zur Geschichte der psychoanalytischen Bewegung.
- (GW-X:126-136) = Erinnern, Wiederholen und Durcharbeiten.
- (GW-X:234-246) = Mitteilung eines der psychoanalytischen Theorie widersprechenden Falles von Paranoia.
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- (GW-X:264-303) = Das Unbewusste.
- (GW-X:402-410) = Über Triebumsetzungen, insbesondere der Analerotik.
- (GW-XI) = Vorlesungen zur Einführung in die Psychoanalyse.
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 (SE-IV/V) = The Interpretation of Dreams.  
 (SE-VI) = The Psychopathology of Everyday Life.  
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 (SE-VII:269–280) = My Views on the Part Played by Sexuality in the Aetiology of the Neuroses.  
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 (SE-X:1–150) = Analysis of a Phobia in a Five-Year-Old Boy.  
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# Semi-Analytic Index

## General Instruction

- a. This index is common to vol. I and II.  
Vol. I comprises first to third book, chapter I-XXXV, and §§ 1-759.  
Vol. II comprises fourth to sixth book, chapter XXXVI-LVIII, and §§ 760-1122.
- b. Cross-references are given to paragraphs (or chapters or books), never to pages. Paragraphs are indicated by Arabic numbers, chapters by Roman numbers, and books by letters.
- c. Within each entry cross-references are listed in this order: books, chapters, paragraphs.
- d. The index consists of four sections, which are presented in this order:  
(S-S) *The Semantic Section* (subjects with encompassing headings).  
(A-S) *The Alphabetic Section* (subjects without encompassing headings).  
(N-S) *The Name Section* (names other than patients).  
(P-S) *The Patient Section* (names of patients).  
[Names of alleged incest victims are not listed in the index. They may be sought in the table of contents, chs. XXVIII-XXXIV.]
- e. In the semantic section entries are not necessarily listed in alphabetic order.
- f. As for the specific Scandinavian and continental letters, 'ä' and 'æ' are treated as 'ae'; 'ö' and 'ø' as 'oe'; 'ü' as 'ue'; and 'å' is placed after 'a'.
- g. For the most part, Gattel's patients will be listed in the index only when referred to outside the fourth book.
- h. The term "quotation" shall indicate a strictly literal replication of a text. "Citation" will be used about a non-literal account.
- i. The writer does not pretend to have invariably hit upon the optimal solution as to whether a certain topic should be placed in the S-S or the A-S section. The reader is advised to look at both sections.—The same advice may be appropriate as regards different headings.
- j. All encompassing headings will now be listed:  
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THE PRINCIPLE OF SIMILARITY

THE PSYCHOANALYTIC STANDARD OPERATION PROCEDURE

THE ILLUSION OF SEPARATION

THE POSTULATE OF THE OUTGROUP

THE DOCTRINE OF OVER-CAUSALITY

THE GOSSIP THEORY OF (PSYCHIC) DISEASE

THE PRINCIPLE OF PRESTIGE

*CONTRADICTIONS* [between methodological claims / observation claims / theories / therapeutic claims]

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*FABULATION (-TECHNIQUES)*

*FORMULAE*

*FREUD*

*INCEST*



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METHODOLOGY, REAL, OF PSYCHOANALYSIS  
METHODOLOGY, ALLEGED, OF PSYCHOANALYSIS  
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344, 368f., 371, 394, 409, 414, 484, 829,  
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#### Nature of observations

- scarce, trivial, non-esoteric, atomistic, available to any untrained lay man and outside any specific observation situation, lacking in devoid of evidential power, easy to render in print, fourth book, 12, 16, 19, 36, 197f., 213–215, 217, 220, 361, 416, 459f., 793, 816–818, 897, 1065f.
- the nature of the secret observations is revealed by the nature of the published ones, XLIII, 211, 254, 361, 525
- contradictory, odd, improbable., 7, 11, 115, 304, 321, 334, 353, 488, 826f.
- oscillation between contradictory positions: psa. observations can easily be adequately rendered in print, and the literature contains a wealth of conclusive observational evidence; vs. psa. observations are so esoteric that it is irredeemably impossible to present them in print, 213, 366

#### Recent and later development:

- observations have become increasingly more scarce in psa. literature, 30–40, 133
- increasingly more empirical content is read into Freud's writings, 134–136, 1112

Secret observations: proving statements by the invocation of, 4, 33, 61, 197, 215, 394, 414, 532–534, 609f., 832

## PERSUASIVE TECHNIQUES

(Persuasive techniques directed against the patient are listed in A–S: suggestive ...)

In the S–S section two sub-divisions are presented, viz. persuasive techniques directed against the reader vs. persuasive techniques related to methodology. The boundary is not clear-cut, and the reader may sometimes search with profit within both categories.)

Almost all persuasive devices have been invented by Freud himself; his followers have mechanically plagiarized them for a century, 932

## PERSUASIVE TECHNIQUES DIRECTED AGAINST THE READER

(see also S–S: fabrication + Freud, merits / A–S: marionette theory)

### [general aspects:]

The principle of paradoxal return

- defined, 32, 769
- 956

Personal psychoanalysis: “individuals who have not undergone a p. ps. are incompetent of criticizing psa.”, 95–97

Attempts at saving psa. are camouflaged as attacks upon psa., XXIII, 229–235

### [specific devices:]

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Criticism: refusal to answer c.s., 201, 207

Disinformation and conceptual confusion, 232, 1065–1070

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- carefully worked out, 261f., 911, 1022
- elegant f.s. without logical content, 25, 140, 209

Hidden reservations, 5, 258f., 261f., 265, 332, 344, 415, 895, 959, 1022, 1068f.

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- feigned, 298, 1046, 1113f.
- Glymour's misinterpretation of Freud's surprise claims, 1113f.

Other devices or sets of devices, V, XIX, XXIII, VI, 27, 34, 196, 219f., 360, 969, 1022, 1027–1029, 1042–1047, 1050, 1087

## PERSUASIVE TECHNIQUES RELATED TO METHODOLOGY

(see also S–S: arguments + metamethodological ... + methodology, alleged ... + psa., later and recent development of)

### Boasting

- substituting the presentation of evidence with boasting of being in the possession of a wealth of evidence of absolute conclusive power; boasting of complete objectivity and total absence of prejudice, IL, 3, 32, 139f., 158, 197, 223, 226, 264, 292, 302, 375, 394, 397, 418, 430, 462, 489, 500, 503, 897f., 906, 1023
- the tables of superlatives, 375, 909
- “only psa. can explain or could have predicted the phenomenon”, 322
- it is always easy for Freud to present unassailable evidence; only, because of a never ending series of excuses he can never do this in the paper at hand, 404f.

[criticism.] Stating in the same paper both the deliberate untruth and its true counterpart; all further reasoning proceeds as if the true statement had never been mentioned; but the isolated presence of the truth will enable psa. to escape or rebuke criticism directed against the false statement, 255–265

### Evidence

- see also: boasting
- claiming or insinuating that the fabricated observations were well-known long before psa.; thereby escaping the obligations of proving them, 911
- “all nontrivial observations are found outside psa. writings”; see S–S: argument: writer’s cramp
- deliberate vagueness, XVII, 25, 158, 258f., 336f., 349, 443, 445, 458, 832, 895, 959f., 1022
- gradual introduction of fabrications, 16f., 47–50, 131, 303, 442, 447f., 1041
- intertwined layers of fabrications which the reader can hardly disentangle, 401, 413

- putting one’s fabrications into the mouths of other people, 6, 74, 203, 293–298, 317, 350, 423, 492, 1034

Fabrications; see: evidence

Fancy-dress costume: putting on the f. of the meticulous methodologist, 94, 102, 775, 906, 913

### Feigning

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- to have previously held a quite different view, but to have been forced by the facts to accept one’s present view, 881

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- defined, 907

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- see also A–S: citation

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(see also S–S: methodology, all sub-divisions including metamethodological + persuasive techniques, both sub-divisions + interpretations + Freud)

[*general nature of psa.*]

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(see also S–S: Freud, thinking ...)

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Increasingly more empirical content is read into Freud's writings, 134–136, 1112

"Criticisms of Freud are beside the point because of later development of psa.", 132

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The postulated therapeutic effect has been continually diminishing during a century, 848–853

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